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Notices of the Aëronautical Society of Great Britain.

February Elections.

Members: Lieut. F. V. Holt, Herman Shaw, and Richard T. Gates.
Associate Member: Norman C. Spratt.
Student: C. Roland Taylor.

March Elections.

Members: F. S. Barnwell, Capt. W. D. Beatty, R.E., G. K. B. Elphinstone.
Associate Members: F. D. Brooker, A. E. Steele, C. H. Vickers.

April Elections.

Members: Christian Gray, Lieut. Baron T. James.
Associate Member: Alfred Curtis.
Foreign Members: Dr. J. C. Rodger, Th. D. Rodger.
Student: C. G. Grinsted.

Council, 1914-15.

The Council for the year 1914-15 is composed as follows:—Associate Fellows: A. E. Berriman, Griffith Brewer, Harris Booth, J. H. Ledeboer, Archibald R. Low, F. K. McClean, Alec Ogilvie, Mervyn O'Gorman, C.B., F. Handley Page, Dr. T. E. Stanton, Lieut.-Col. F. H. Sykes, Dr. A. P. Thurston. Members: Eng.-Lieut. G. Aldwell, R.N., Col. H. E. Rawson, C.B., R.E., Major-General R. M. Ruck, C.B., R.E. (ret.), Dr. R. Mullineux Walmsley.

Election of Chairman.

Major-General R. M. Ruck, C.B., R.E., has been elected Chairman of Council for the year 1914-15.

Appointment of Committees.

The following Committees have been appointed:—

(1) Inventions Committee: T. W. K. Clarke, Col. H. E. Rawson, and B. G. Cooper.

(2) Library Committee: H. F. Lloyd and B. G. Cooper.

(3) Research Committee: HARRIS BOOTH, T. W. K. CLARKE, B. G. COOPER, Col. J. D. FULLERTON, B. MELVILL JONES, ARCHIBALD R. LOW, MERVYN O'GORMAN, C.B., F. HANDLEY PAGE, Dr. A. P. THURSTON.

(4) Finance Committee: Major-General R. M. RUCK, Dr. A. P. THURSTON, ARCHIBALD R. LOW, the SECRETARY.

(5) Technical Terms Committee: L. BAIRSTOW, HARRIS BOOTH, A. GRAHAM CLARK, T. O'B. HUBBARD, ARCHIBALD R. LOW, MERVYN O'GORMAN, C.B., Dr. T. E. STANTON, Lieut.-Col. F. H. SYKES (representing the War Office), Lieut. E. D. M. ROBERTSON, R.N. (representing the Admiralty).

Alteration of Programme.

The papers originally contemplated for February 18 and April 1 were, owing to unforeseen circumstances, not forthcoming. For February 18, however, Mr. F. H. Bramwell very kindly recast the paper on "Propellers" he had in preparation for the Research Committee, and on April 1 Messrs. B. C. Hucks and C. Gordon Bell contributed papers on "Three Years' Flying Experience."

Co-optation of Council Members.

In place of Colonel J. E. Capper and Mr. J. W. Dunne, resigned, Mr. Harris Booth, B.Sc., A.F.Ae.S., and Mr. A. R. Low, M.A., A.F.Ae.S., were co-opted.

Lectures at the Aero and Marine Exhibition, Olympia.

The Society organised a successful series of explanatory lectures (one each day) during the course of the Aero Show. The programme is given below, and the Society is very much indebted to all the gentlemen who kindly assisted to make the series a success.

Date.	Time.	Title.	Lecturer.
March.			
16.	7.30 p.m.	"How an Aeroplane is Built." ...	Mervyn O'Gorman, C.B., M.I.M.E., A.F.Aë.S., Superintendent Royal Aircraft Factory. Marquis of Tullibardine in the Chair.
17.	,,	"How an Aeroplane Lifts"	Bertram G. Cooper, A.F.Ae.S.
18.	3.30 p.m.	"How an Aero-Engine Works"	A. Graham Clark, M.I.A.E., Technical Editor of "Flight."
19.	7.30 p.m.	"How an Aeroplane is Steered and Controlled." ...	Dr. A. P. Thurston, D.Sc., A.F.Ae.S.
20.	,,	"How an Aeroplane Upsets" ...	Leonard Bairstow, A.R.C.Sc., National Physical Laboratory.
21.	,,	"How Aircraft are used in War" ...	Lieutenant-Colonel F. H. Sykes, A.F.Ae.S., Officer Commanding Military Wing, Royal Flying Corps.
23.	,,	"How an Aeroplane is Propelled" ...	F. H. Bramwell, B.Sc., A.F.Ae.S., National Physical Laboratory.
24.	,,	"How an Airship Works" ...	Captain C. M. Waterlow, R.E., A.F.Ae.S.
25.	,,	"How a Waterplane Works" ...	G. Holt Thomas, E. C. Gordon England, T. O. M. Sopwith.

Wilbur Wright Memorial Lecture.

The second "Wilbur Wright Memorial Lecture" will be delivered by Dr. R. T. Glazebrook, C.B., F.R.S., F.Ae.S., on "The Development of the Aeroplane," at the Royal United Service Institution, Whitehall, on Wednesday, May 20th, 1914, at 8.30 p.m.

The Right Hon. Lord Sydenham, G.C.M.G., F.R.S., will preside.

On the evening preceding the Meeting (viz., on the 19th May) the Council will entertain the Lecturer and other distinguished guests at a Banquet, at the Royal Automobile Club, Pall Mall, S.W., at 7.45 p.m. for 8 p.m.

Members are invited to purchase tickets for themselves or their friends at £1 1s. each, inclusive of wines, cigars, etc. Members desirous of contributing to the guarantee fund, which has been opened to defray the costs of official guests, may send subscriptions in addition to purchasing tickets.

Cheques should be made payable to Griffith Brewer and sent to the Aeronautical Society, 11, Adam Street, Adelphi.

Library.

The Council desire to acknowledge the following gifts to the Library:—"Transactions of the Institution of Engineers and Shipbuilders, Scotland," Vol. LVI., 1912-13. "Bulletin de l'Institut Aerotechnique de l'Université de Paris," Fascicules I., II. and III. (presented by M. Maurain). "Some Forms of Scientific Kites," E. Stuart Bruce. "The Automatic Release of Self-Recording Instruments from Ballons sondes," E. Stuart Bruce. "Mechanical Flight," E. Stuart Bruce.

The FIFTH MEETING of the FORTY-NINTH SESSION of the Aeronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 21st January, 1914, at 8.30 p.m. Sir Alfred Keogh, K.C.B., presided.

THE STABILITY OF AEROPLANES.

(Illustrated by Experiments with Model Gliders.)

BY LEONARD BAIRSTOW, A.R.C.SC.

The problems which arise in the course of a study of aeroplane stability are of considerable complexity as compared with those confronting engineers in other branches of locomotion. Ocean-going vessels provide another instance of the motion of a solid body in a fluid and their stability is clearly one of vital importance. Naval architects have consequently studied the subject and calculations are made for the stability of each new design of ship. The process is so well known and so well founded that it surprises no one that a newly launched vessel remains on an even keel. One instance, not connected with British shipbuilding, serves to remind designers of the serious consequences of faulty calculation. The disaster relates to a ship which was launched fully engined, etc., and which turned turtle and sank within a few minutes of launching.

The type of stability which the naval architect needs to calculate most carefully is that which is concerned with the security of the ship when rolling. The flotation or stability in a vertical direction is comparatively easy to obtain, and pitching, although it may tend to discomfort, is not likely to lead to disaster.

In marine engineering probably the motion of a submarine, when submerged, presents problems most nearly approaching those of aeronautics, but the approximation is rather to the dirigible than to the aeroplane. The aeroplane is the only man-carrying apparatus which must be in rapid motion before it can fulfil its function as a weight carrier. In the first place, then, it is obviously necessary to maintain the speed of flight, but this is not a sufficient condition of safety. An aeroplane may fail in other ways than by failing to maintain its speed, for it may roll over, turn tail foremost, or pitch completely over. Various combinations of these motions may occur and the nature of the failure to maintain a steady motion then becomes somewhat complicated.

In all probability difficulties in respect to stability limited the duration of the early flights of Santos Dumont, Farman, Bleriot, etc. It may be said that the controls were imperfect before the Wright Bros. introduced their system of wing-warping in conjunction with rudder action, and that this deficiency in control would be sufficient to account for the partial failures of the early aviators. Although this objection may hold good, it will surely not be contended that a machine which is totally dependent on the skill of the pilot for its safety is as good as one which can right itself without the pilot's assistance.

What is a Stable Aeroplane?

The following definition of a stable aeroplane is proposed as embodying the above expression of opinion:—

"A stable aeroplane may be defined as one which, from any position in the air into which it may have got either as the result of gusts or the pilot's use of the controls, shall recover its correct flying position and speed when the pilot leaves the machine to choose its own course, with fixed or free controls, according to the character of the stability."

Sufficient height above the ground is presumed to allow an aeroplane to reach a steady flying state if it is able to do so. The more rapidly the aeroplane recovers its flying position the more stable it may be said to be.

If a pilot is necessary in order that an aeroplane may return to its normal flight position, then the aeroplane itself cannot be said to be stable, although the term may be applied to the combination of aeroplane and pilot.

Inherent and Automatic Stability.

A subdivision of stability is useful, the terms " inherent " and " automatic " being already in use. An aeroplane is said to be " inherently stable " if, when the controls are fixed in their normal flying position whilst the aeroplane is in any position and flying at any speed, the result is to bring the machine to its normal flying position and speed. " Automatic stability " should be used only to describe stability obtained by a mechanical device which operates the controls when the aeroplane is not in its correct flying attitude and so rights the flying machine.

Although the subject of stability may be thus subdivided, it does not necessarily follow that the methods used for producing inherent stability do not throw light on the requirements for automatic stability devices. It can be seen axiomatically that before a designer is in a completely satisfactory position he must have information which will enable him to find the motion of an aeroplane under any conceivable set of circumstances. The same information which enables him to calculate the inherent stability of an aeroplane is also that which he uses to design effective controls and the same as that required for any effective development of automatic stability devices.

It may be said that a designer can never tell the nature of the gusts which his aeroplane will have to encounter and therefore cannot anticipate the consequences to the flying machine. In this respect he is only in the usual position of the engineer who uses his knowledge to the best of his ability and admitting his limitations provides for unforeseen contingencies by using a factor of safety. Nevertheless, in spite of such limitations the mathematics is not, as is often supposed, unable to deal with the problems arising from flight in a gusty wind.

Inherent Stability.

Having defined inherent stability, an attempt will be made to show, by means of a model, that it is possible to design a flying machine, not carrying a pilot, which will take up a correct flying attitude and speed, no matter what its original attitude and speed may be, *i.e.*, no matter how it is launched or thrown into the air.

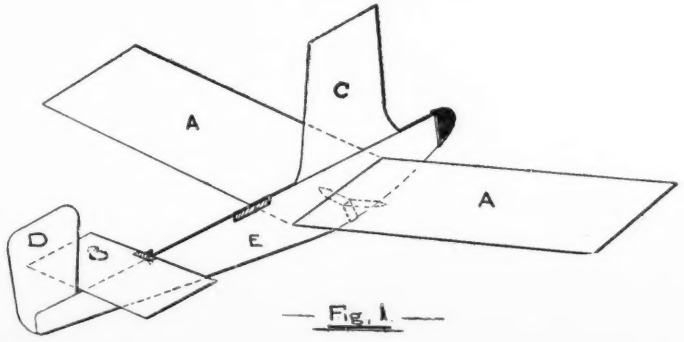

Fig. 1.

The model is illustrated in Fig. 1 and contains all the essential features of an aeroplane except the engine. The flights will then be gliding flights, the energy being obtained from gravity. Only in respect of certain effects of side winds, etc., on the forces due to the propeller does the problem of the glider differ from that of the stability of a power-driven aeroplane and the differences are differences of detail and not of principle.

Model Demonstrations.

The model may be thrown into the air in any way whatever, and provided only that no object is struck in the meantime, will soon be flying correctly. To show the effects without danger of collision with objects in the room a clear drop of 15 to 20 feet is required with about 30 feet clear in every direction. The flights which will be attempted are:—

(*a*) Simple launching in approximately the correct attitude and at the proper speed.
(*b*) Dropping with the model right side up.
(*c*) Dropping with the model inverted.
(*d*) Dropping with the wings vertical.

The latter is the most likely to end in collision with objects in the room and more than one attempt may be necessary before an uninterrupted flight is obtained. (Flights.)

The model, which is made of mica, is not put forward as a copy of an aeroplane, but as an aeroplane itself, just in the same way that a house fly may be called a living flying machine, although it is many times smaller than an eagle or an albatross. Large scale experiments cannot be shown to you for obvious reasons and on account of the danger to the pilot it is difficult under any circumstances to carry out, with a man-carrying machine, experiments of the kind which will be attempted this evening.

The assertion is however made here that the mathematical reasoning applies equally well to all flying machines irrespective of their size and shape.

The remaining models are designed to show the prevalent types of instability to which a present day flying machine is liable. The manufacture of the models is based on certain mathematical equations in which nothing is new but their application to an aeroplane. Laplace used similar equations to examine the stability of the solar system and Lord Kelvin applied them to the study of gyroscopes. The method was finally standardised by Routh, the famous Cambridge coach, and was first applied to aeroplanes by Prof. Bryan in his book, "Stability in Aviation." Another mathematical method of attacking the problem of aeroplane stability has been developed by Mr. Lanchester in his books on "Aerial Flight," and was the first method successfully used to calculate the conditions of stability of an aeroplane. The method bears little superficial resemblance to that used by Bryan, but since there is only one set of laws of motion it must necessarily happen that if correct both methods will lead to the same results. This is found to be actually the case, and to both authors the speaker is greatly indebted for the assistance obtained from their books.

Effect of Gusts.

The model which has been flown as an indication of what may be expected of an inherently stable machine has had the advantage of flying in comparatively still air. The objection is often raised that the calculations presume still air and neglect the existence of gusts. The objection is plausible and yet quite untenable when examined carefully. For instance, the mathematics includes a

term for the effects of side-slipping of the aeroplane. Exactly the same term applies if the aeroplane continues on its course but receives a gust from the side. A head gust and an upward wind are similarly contemplated by the mathematics, and it is impossible to conceive a gust of such a complicated nature that the mathematics does not provide the mechanism for examining the effects on the motion of an aeroplane.

Given sufficient head room, an inherently stable aeroplane will weather any gust it may receive. In doing this, it will roll, pitch, change its course and speed, etc., so that any disturbance will die out in a time of, say, 20 to 30 secs. Successive gusts simply add their effects and no matter what the aeroplane has been doing previously its motion at any instant will not depend appreciably on anything but the gusts encountered in the last half minute of its flight. In this time an aeroplane would have travelled half a mile or more and it may be contended that much may happen in this time that is undesirable. The rate of reduction of the disturbance might be increased if desired, but a problem of an entirely different kind then becomes important.

"Weathercock" Stability.

A stable machine, whether the stability be inherent or automatic, must turn into the relative wind more and more rapidly as the stability is increased and therefore a very stable machine will be tossed about in a wind more than a less stable machine. If the stability is too great the result will be discomfort to the pilot.

Utilisation of Gravity Effects.

A distinction is here of importance; existing machines turn into the relative wind with great rapidity. The slow motions tending to restore the machine to its correct attitude arise from the inclinations of the machine relative to the earth, and it appears probable that these slow motions may be made quicker without at the same time increasing the rapidity of the already existing rapid motions. In other words, it appears that the stability of modern aeroplanes might be greatly increased without introducing any further discomfort to the aviator.

It follows from these remarks that a useful automatic stability device should be gravity controlled, either directly or indirectly. Any device which does not satisfy this condition can only be expected to regulate the motion of the aeroplane relative to the air, and as this is not in itself a difficult stability problem, its use will be confined to securing greater comfort for pilot and passengers.

Enough has now been said to indicate that the mathematical method is very comprehensive and can include within its bounds almost all the stability problems which confront the designer of an aeroplane. An attempt will now be made to indicate experimentally some of the consequences of failure to obtain stability, and then to draw attention to those features of an aeroplane which are of primary importance in their effects on its stability.

Longitudinal Stability.

In connection with the disturbances which give rise to the problems of longitudinal stability, broad distinctions have been made by Mr. Lanchester, and the subject is thus divided into three sections, which may be briefly referred to under the heads:—

(*a*) Catastrophic instability.
(*b*) Rapid oscillations.
(*c*) Phugoid oscillations.

The first of these has been described by Mr. Lanchester at the recent meeting of the British Association in Birmingham.

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Catastrophic Instability

Is concerned particularly with the effects on an aeroplane of upward or downward gusts. In certain cases the attitude of the machine in steady flight may be completely altered by a sudden vertical air-current. The new flight position is usually upside down and should it occur without the pilot being strapped to his seat he would inevitably be thrown out. If strapped in, the pilot may recover his alternative position of steady flight by the use of the controls.

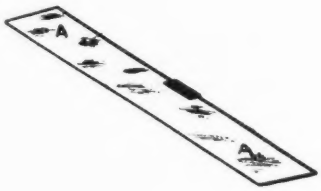


Figure 2.

Catastrophic instability may be illustrated by a model similar to that of Fig. 2. Launched in one way the model flies quite well as an ordinary glider. A different setting of the launching apparatus produces a quite different result, the model dips suddenly, turns over, and continues its flight in the direction opposite to that in which it was launched. (Flights.)

Catastrophic instability is not very difficult to avoid, the sole condition to be satisfied being, " that for any one setting of the elevator there shall not be more than one position for which the pitching moment about the centre of gravity shall be zero."

Rapid Oscillations.

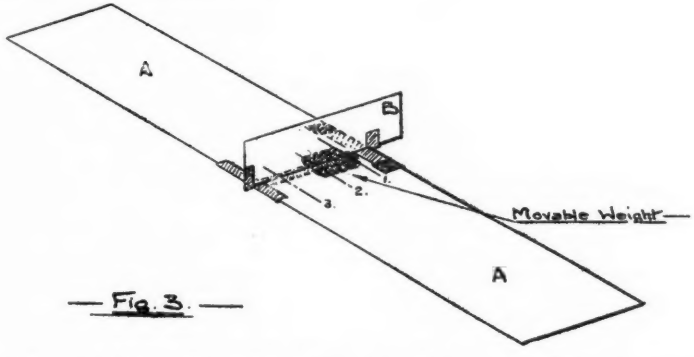
The rapid oscillations of an aeroplane depend almost entirely on the forces and couples arising from an inclination of the relative wind to the direction of flight and are scarcely affected by the earth's attraction. The rapid oscillations almost always die down, but a flight will be shown in which it appears that they develop into a steady rotation of the model. In certain cases the rapid oscillations change into a dead beat motion which cannot give rise to a dangerous motion of the aeroplane.

Phugoid Oscillations.

For actual aeroplanes the phugoid oscillations are comparatively slow, the period being about 20 secs. The aeroplane is stable if the amplitude of any oscillation decreases and this effect can always be produced by using a sufficiently large tail plane and elevator. If the moment of inertia of the aeroplane is large and the tail not quite great enough the phugoid oscillation increases its amplitude and the aeroplane will rise and fall in a wave of increasing height, ultimately striking the ground awkwardly. It has been contended in France that this motion is so slow that the pilot is not troubled by it, but it cannot be too strongly insisted upon that phugoid instability is undesirable as throwing a quite needless strain upon the pilot.

Models Illustrating the Rapid and Phugoid Oscillations.

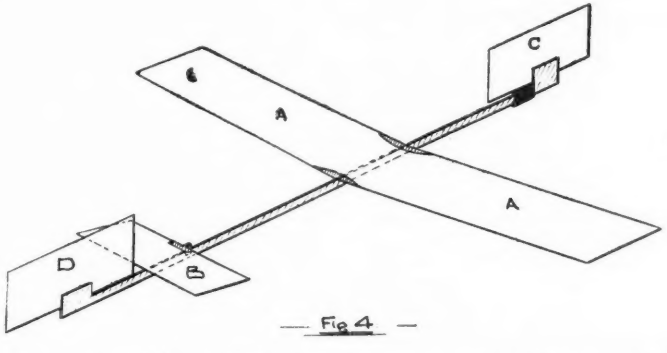
Figs. 3 and 4 give a general idea of the models which will be used to illustrate longitudinal stability calculations.



— Fig. 3. —

The first model is only a little different to that used to illustrate catastrophic instability and consists essentially of a loaded mica plane. A movable weight allows a certain amount of adjustment of the position of the centre of gravity.

With the centre of gravity well forward, *i.e.*, one-third of the chord from the front, the flight of the model is stable and only a damped phugoid can be detected in the motion. (Flight.) With the centre of gravity a little further back, the rapid oscillation can be seen to be superposed on the slower phugoid. This rapid oscillation is not readily seen from all positions. (Flight.) If the centre of gravity is moved still further back until it is in the centre of the chord, the model turns over and over backwards and glides down at a steep angle. (Flight.)



— Fig. 4 —

To show the unstable phugoid oscillation the model illustrated in Fig. 4 will be used. It differs from the weighted mica plane in its greater moment of inertia and an elevator plane is necessary to produce stability. At a certain inclination of the elevator plane the model becomes unstable and the model shows the increasing wave which is the first indication of longitudinal instability. (Flight.)

A further decrease in the inclination of the elevator plane would produce a steady nose dive. A ballasted mica plane will however be used to demonstrate this motion as the damage to a more complicated model is often sufficient to spoil it. (Flight.)

Remarks on Producing Longitudinal Stability.

Longitudinal stability is not difficult to secure and is generally identified with the principle of the upward Vee. Only one organ, the tail plane, need be altered to produce stability, and the principle of the upward Vee is expressed by the statement that (independently of their relative size) the forward plane shall be more heavily loaded per square foot than the rear plane.

Inherent longitudinal stability tends to maintenance of speed, and a stable machine must settle down to a definite speed for each setting of the elevator. Shutting off the engine without adjustment of controls has little effect on the steady speed, the additional power being obtained from a fall under the influence of gravity. The change from horizontal to gliding flight with the engine stopped usually only involves a speed variation of about 1% when the resulting phugoid has been damped out.

Lateral Stability.

Lateral stability appears to be more difficult to obtain than longitudinal stability and the disturbed motions are of a different character.

One of the motions is essentially dead-beat in any heavier than air machine. The motion is so rapid and transient that a model in actual flight cannot be used for a demonstration. The salient features can however be easily understood without such assistance. If we imagine that this model of an aeroplane is given a rolling motion when in flight by a quick outward and return motion of a miniature warp lever, then the subsequent motion will be as follows:—The right wing is rising and consequently the air strikes it at a reduced angle of incidence and the lift on it is reduced. Conversely the lift on the left and downward moving wing is increased and the combined effect of the two forces is to introduce a powerful couple tending to stop the rolling. The rolling ceases with enormous rapidity and is practically over in a fraction of a second. The rolling motion, however, simply stops and the couple described does not restore the machine to an even keel. This restoration takes place in consequence of secondary actions introduced by side-slipping under the influence of gravity.

Motion which becomes a Spiral Guide when Unstable (Spiral Instability).

This motion can be illustrated by a model and is one of the motions which may be dead-beat. Unlike the rolling described above, the disturbance may increase progressively instead of decreasing and is dependent on and controlled by the earth's attraction. Spiral instability arises from the use of too large a rudder, and it is the proportioning of the rudder and vertical fin system generally which presents the most difficult of stability problems.

If the machine is unstable, then on taking a turn to the right, side-slipping occurs inwards to the right after a preliminary outward movement under the action of centrifugal force. The machine then overbanks and the rate of turning is increased. This goes on with increasing rapidity unless checked by the pilot. There exists a limit to the rate at which an aeroplane may increase its banking, but as this only means a period of 15 secs. before the machine becomes uncontrollable, the limit is higher than practical considerations would show to be desirable.

The spirally unstable motion will be shown with a model of the type illustrated in Fig. 4. The elevator will be put in a position which ensures longitudinal stability. The removal of the front fin is usually sufficient to produce the instability. (Flight.)

Oscillation.

The lateral oscillations of an aeroplane are of peculiar importance as it follows from a study of them that "the most stable aeroplane is one which has for one of its types of lateral motion an oscillation." The statement does not go so far as to say that a machine which oscillates is stable and a model will be flown which will show that such a statement would be wrong; it does, however, mean that a machine which does not oscillate has one of its stability factors small.

The oscillation becomes unstable when the rudder is not large enough and thus a limit is obtained opposite to that of spiral instability which arises from the use of too large a rudder. Starting with a stable aeroplane and reducing the rudder more and more has the effect of making the oscillations more and more apparent until finally they tend to increase in size from one wave to the next. The model side-slips and turns, keeping on an averagely straight course, but with increasing banking until it either loses speed and falls or overturns. (Flight.) With sufficient room beneath it an aeroplane would recover, but only to begin again an increasing oscillation.

The model used to illustrate this type of instability has two fins of approximately equal areas.

If the rudder is still further reduced, the front fin becomes the larger and the unstable oscillation gives place to an instability of a very different kind. The model spins round about a vertical axis with great rapidity, loses all its forward speed and falls. (Flight.) From the inversion arising from this type of instability it is unlikely that a model would ever recover, no matter what the height of fall.

Other types of lateral instability are known, but so far as modern machines are concerned the more important types have now been described.

The Important Features of an Aeroplane with respect to Lateral Stability.

The vertical fin system on which lateral stability depends is provided by the side surface of the body, the rudder and the dihedral angle or equivalent vertical fin above the centre of gravity. If a machine is spirally unstable the instability may be removed by deepening the body forward of the centre of gravity, or by increasing the dihedral angle. If the unstable oscillation appears the rudder area should be increased.

All full scale experiments should, however, be carried out tentatively, as the inter-relation between the effect of the rudder and the dihedral angle, in order to produce stability, are not very simple. An aeroplane may be made unstable by increasing the dihedral angle unless the other factors in the stability equations are correspondingly changed.

If carried out with the necessary care (and there is no obvious royal road to success) it is quite certain that any aeroplane whatever which is capable of lifting its load, with some reserve of engine power, can be made laterally stable by attention to this vertical fin system. The details of the resulting motion will depend on the particular way in which the aeroplane is made, but stability in itself does not impose conditions so rigorous that a designer has not ample room for the exercise of his ingenuity for the improvement of efficiency, comfort, etc.

No attempt has been made to discuss the relative merits or demerits of "inherent" as against "automatic" stability. The reasons for this are twofold. In the first place automatic stability is not yet sufficiently developed either practically or theoretically; and secondly, there appears to be room for the development of both systems for use in the same aeroplane but for producing complementary effects. The only reason for the greater attention given to "inherent" stability this evening is that the subject is better known to the author, and it

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appears to him that the mathematical analysis offers suggestions for immediate improvement in the stability of modern aeroplanes.

DISCUSSION.

Mr. Booth maintained that the lecturer had dismissed the effect of the propeller too cursorily, and pointed out that if no provision is made for balancing the propeller torque in an inherently stable machine the pilot is not free to switch on and off without attending to his lateral controls. He also said that Mr. C. R. Fairey had pointed out to him that Bryan's mathematical treatise on stability did not deal with curvilinear flight, and was therefore incomplete, and went on to say that he had endeavoured to supply the deficiency, but had failed; he hoped that a mathematician would be found to supply the solution, as then one could stop talking about stability, and build stable machines instead. Mr. Booth went on to describe a satisfactory method of launching gliding models from a kite. His method is to have an endless kite string, passing through a pulley on the kite bridle and another one on the ground. The glider is attached to one side of the kite string by a piece of cotton thread, and is then hauled up to the kite by pulling on the other side of the kite string; on reaching the kite pulley the model is released by the breaking of the cotton, and is thus projected at random from a good height in the open air under wind conditions.

Mr. T. W. K. Clarke: Among a number of vastly interesting points raised by this paper, there is one to which I will confine myself, and on which I will ask Mr. Bairstow for elucidation.

He said, "existing machines turn into the relative wind with great rapidity. The slow motions tending to restore the machine to its correct attitude arise from the inclinations of the machine relative to the earth, and it appears that these slow motions may be made quicker without at the same time increasing the rapidity of the already existing rapid motions. *In other words, it appears that the stability of modern aeroplanes might be greatly increased without introducing any further discomfort to the aviator.*" And he proceeds, "It follows that a useful automatic stability device should be gravity controlled, either directly or indirectly."

Now, I take this to be as follows:—

Suppose a machine to be flying level and to receive a side gust causing the relative wind, instead of being in the symmetrical vertical plane, to be in a plane inclined to the vertical. The machine would in a very short time, say half a second, set its wings more or less square to this new plane. If now the gust stops, the machine finds itself with a sideways cant, then until a motion of side-slipping has set in the recovery is only brought about by the couple formed by the weight downwards and the equal and opposite vertical component of the air pressure upwards, this latter, until side slipping takes place, being, of course, in the plane of symmetry. This recovering couple is much smaller than the upsetting couple, and hence the recovery is much slower than the upsetting; in fact, even with the helpful action of the side-slipping, the recovery may last, say, five seconds, during which time the effects of other gusts may be accumulating, as against the half second for the upset. As this latter, say 10° in half a second, is the measure of the pilot's discomfort, I take it that Mr. Bairstow suggests that means might be taken to speed up the recovery without adding to the discomfort of the pilot. In fact, if the time of recovery is reduced, one might say that the number of gusts per recovery is reduced, so that the total upset angle is reduced, and the comfort of the pilot in this way bettered. Anyhow, I think that this suggestion of Mr. Bairstow's deserves very great attention. Mr.

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Bairstow continues by suggesting that as when the machine is first canted over all the air pressures are symmetrical relative to the machine, but that gravity is not, therefore any automatic control must be gravity operated. This is supposing that there is not a third alternative. But there is the alternative of employing an apparatus which is operated by the actual motion of upsetting, viz., an inertia controlled apparatus, such, for instance, as a bar with weights at each end mounted on an axis running fore and aft. Another such apparatus and one more powerful for its weight is, of course, the gyroscope. I admit that as, with such an apparatus, owing to friction, etc., it is impossible to maintain absolutely a given direction (it might, for instance, be set vertical by hand at starting), a pendulum might also be employed to recover this loss. Perhaps our able lecturer will include the above among his suggested methods.

Mr. F. HANDLEY PAGE showed a small model of a stable machine, which could be made to loop the loop when launched with sufficiently high velocity. If the velocity were not quite high enough, the machine would come to a stop when about to reach the top of the loop, and would subside backwards in a tail slide.

Major BROOKE-POPHAM: The really important thing, from the pilot's point of view, is to have an aeroplane that will recover its normal flying position in the smallest possible time, or rather in the least possible vertical distance.

It does not really matter very much to a pilot what happens to his machine when 1,000 ft. up in the air; what does concern him is the position of the machine when near the ground.

At present it would appear that no form of inherent stability possesses anything like the same amount of righting power as the ordinary forms of control, and I think that no form of inherent stability should be used, if doing so involves the sacrifice of part of the controlling power of the pilot.

Mr. A. R. Low said that he did not feel himself sufficiently instructed in the very difficult subject to take part in a detailed discussion.

He hoped in the course of a year or two to have adequately digested the whole theory of small oscillations, with Routh's discriminant functions **A**, **B**, **C**, **D**, **E**, **H**. Bryan's special forms of them, derived from his assumed empirical equations for the coefficients of lift and drag, and the variations in these special forms which would arise from the introduction of more accurate empirical formulæ for coefficients of lift and drag, and for the rotary derivatives.

When he had digested these, in the sense of being able to carry out detailed calculations for full-sized machines with confidence in his grasp of the theory and certainty in his arithmetic (a certainty only born when practice and theory had checked each other repeatedly) he would then take part in discussing such a paper as the admirable one presented that night.

But he might make the general remark that mathematical methods were absolutely necessary.

He was glad that Mr. Harris Booth, whom they had to congratulate on his appointment as civilian technical adviser to the Admiralty, had said, not that less, but that more mathematics was wanted, to extend still further the remarkable results which flowed from Professor Bryan's method.

Mr. Low made the suggestion that no one was better able to carry out such work than Bryan himself, and hoped the Advisory Committee would adopt it and would persuade the Admiralty or War Office to offer adequate inducements to Professor Bryan to carry out the work.

Then, as Mr. Booth has said, instead of talking about stable aeroplanes, manufacturers in this country would be put in a position to make stable aeroplanes.

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He could not refrain from alluding to a very ignorant agitation which was being carried on in parts of the lay Press, and he was sorry to say in a certain section of what he might call the technical Press, against the organisations directly connected with the Government. He could only offer the considered opinion, that the writers who took up this line were not competent to understand the enormous advances made at the National Physical Laboratory and at the Royal Aircraft Factory towards reducing the technical problems to a systematic routine, and toward introducing standards in manufacturing.

Some years ago, so-called "practical" engineers had laughed Professor Bryan out of the British Association meeting, when he claimed that applied mathematics would save pilots' lives.

That evening Mr. Bairstow had shown them a series of models which had one of the discriminant functions $\mathfrak{A}, \mathfrak{B}, \mathfrak{C}, \mathfrak{D}, \mathfrak{E}, \mathfrak{H}$, a small negative quantity, and to each negative discriminant corresponded a special type of instability. That constituted a triumphant vindication of Professor Bryan, and of the Advisory Committee in obtaining the services of men like the lecturer, who were able to demonstrate the value of the results predicted by abstract methods to the eyes of the least competent observer.

He hoped that the lesson would not be lost on the journalistic type of critic of engineering matters, and that they would see, however faintly, that there were problems in aviation which they had not begun to be able to understand, and which were being successfully reduced by the joint work of the N.P.L. and the R.A.F. staffs.

Mr. E. T. Busk: Referring to Major Brooke-Popham's remarks, I consider it unlikely that it will be possible in the near future to make aeroplanes which will be safe near the ground without human control. The fifty feet suggested by Major Brooke-Popham is much too small a vertical allowance for the waves of the flight path. I consider it therefore absolutely essential that the controls should be powerful enough to overcome all natural couples, since the manœuvres of landing and starting often necessitate the presence or absence of angular motions contrary to those which would occur in uncontrolled flight. Absolute control must therefore be allowed to the flyer so long as he keeps the speed high enough for the angle of the planes to be less than their critical angle. At a lower speed—*i.e.*, when the aeroplane is pancaking—such control cannot at present be afforded.

It has often been remarked that pilots are opposed to natural stability. It appears that the reason for this is either that unstable machines are difficult to fly, and it is human nature to like to do that which is difficult, or that they lack experience of stable aeroplanes. Experiments in longitudinal stability have shown that a slightly stable aeroplane is not only more comfortable to fly but easier to land than one which is unstable.

An aspect of the stability question which has not yet been seriously considered is that of the power loss. The side-slip necessary to right the roll involves loss of power. This has not yet been calculated, but on its amount will depend to a great extent the selection of a system.

Another point which has often escaped observation is the fact that at present all stability arrangements, whether inherent or mechanical, depend on gravity for a sense of direction. This fact makes the essential characteristics of all devices similar, side-slip being as necessary for mechanical as for inherent lateral stability.

Mr. Mervyn O'Gorman referred to the desirability of ascertaining from model experiments, such as had just been witnessed, what would be the evolutions of full size machines similar to the models. He pointed out that the disadvantage of

any large vertical fin system, such as that possessed by Mr. Bairstow's stable model, was that such a surface made landing more difficult in a side wind. It was important to remember that a machine which appeared disturbed in a gusty wind was not necessarily unstable. Oscillations were necessary for recovery, and such a machine might be very stable, although the unpractised observer would say that it was unstable because it did not remain unaffected by gusts.

Mr. B. MELVILL JONES: Some time ago I was speaking to a pilot of considerable experience who is a strong opponent of inherent stability. His principal objection to it seemed to lie in the fact that a machine which possesses great inherent stability is of necessity considerably affected by wind gusts, and is, as a rule, comparatively slow in answering to the controls. He argued that the unconscious reflex actions necessary to maintain a neutral or even an unstable machine in equilibrium are less fatiguing than the conscious efforts required on a very stable machine to maintain a good course across country on a gusty day, owing to the continual necessity to readjust the direction of flight after the machine has been turned aside by a gust.

This objection to a stable machine brings up what I consider to be the most important question before the aeronautical world at the moment, the question of the amount of stability that a good aeroplane should possess, for I think you will all agree with me, after listening to the lecture to-night, that the possibility of obtaining complete inherent stability by means of small modifications only, on almost any existing type of machine, is now established beyond a doubt. This problem must, of course, be settled ultimately by the pilots themselves, since it depends primarily upon the extent to which the human body can be trained to perform the necessary balancing movements unconsciously, and without undue fatigue.

I myself am of the opinion—and I believe that the lecturer agrees with me—that slight positive stability only in all directions will be the ultimate desideratum for most aircraft, as it is for almost every other known type of dynamic machine, for the only machine that will not turn about any axis under the influence of a gust is one that is in neutral equilibrium. Any departure from neutrality, either in the direction of stability or instability, necessarily involves a certain amount of sensitiveness to gusts.

It might, however, be possible to combine both the advantages of stability and neutrality in a single machine by making the amount of stability variable at the will of the pilot. This would not be at all difficult from the structural point of view, and a machine so equipped might be set to have considerable stability when high up on calm days, and so relieve the pilot of a certain amount of strain, and to be nearly neutral when close to the ground or on gusty days, when rapidity of control, and independence of wind gusts, become matters of paramount importance.

Colonel FULLERTON, *R.E. (communicated)*: There are a few other points upon which the lecturer may perhaps be able to give us some information, viz.:—The effect of the "angular resistances," the influence of the loading, and the connection which should exist between the designs of the wings and the body.

Angular Resistances or rotary derivatives.—These may safely be neglected, but as they, under certain circumstances, seem to improve the stability, I should like to know the result of any experiments made to test the extent of such improvement.

Loading.—Some of the older aviators maintained that their models flew better when the load per square foot was a certain amount (fixed for each model). This seems only natural, as the wings, etc., are designed to have certain definite stream line systems when loaded in a particular way. When the loading (and

consequently the velocity) is altered, the system of stream lines is also changed, and it seems probable that this has some effect on the stability. [Some light might be thrown on this by pilots who have made very long flights, and expended a considerable amount of fuel, etc.]

Design of Wings and Body.—At present constructors seem to fit up almost any kind of body, the chief matter considered being the reduction of the resistance. A study of birds shows that there is a definite connection between the forms of the wing and the body and a corresponding connection between their resistances. The result of these arrangements is to make birds very stable longitudinally, and their other forms of stability seem to be favourably affected also. Perhaps the lecturer can give us some information on these two latter points.

Dr. A. P. THURSTON (*communicated*): I have often been asked by aeroplane builders and designers, "What's the use of models?" when I have mildly suggested that they might gain information by constructing a small model of the machine they were proposing to build. This question has been most splendidly answered by Mr. Bairstow's beautiful models, and I venture to suggest that, simple though the paper is, there is much thought for even the most scientific designers. This paper is of the type which not only instructs—it does more—it sets one thinking, and that is a great achievement. I have been able on several occasions to demonstrate the fallacy of certain ideas and the unsuitability of various designs of machines by constructing models, and have thereby prevented much money being wasted, and—what is of more importance—have prevented brave lives from being risked unnecessarily. I am therefore a great believer in the virtue of model making and experiment.

As the Chairman of the Research Committee of the Kite and Model Aeroplane Association, I most warmly welcome this paper, and would congratulate Mr. Bairstow upon it. I have no doubt that he could, had he so desired, have given us a highly mathematical dissertation on the subject, and have bored us beautifully. He has that kind of look in his eye; but he has been good enough to spare us, and in doing so I think he has been wise, because, when all is said and done, many experiments have to be made and much information collected before we shall be in a position to apply with certainty a large amount of mathematics to the design of flying machines. Common sense and "rule of thumb" methods appear to be the only safe guides at present. The mathematical solutions of the problem of stability are of most use in pointing our minds to the ideal conditions and in indicating the "awful examples" to be avoided. This the paper has done in a very clever and simple way.

It is possible, for instance, to design a machine which is quite stable as a glider, but which is unmanageable when the propeller is running. In other words, the back-draught of the propeller will entirely upset all the calculations, and it is most difficult to suggest suitable coefficients allowing for this. Other machines are quite stable when the propeller is running, but unstable when the power has been cut off.

It should be remembered that stability and efficiency act in opposite directions; moreover, too much stability gives a "hunting" or "weather-cock" effect. Hence it is not desirable to give a machine a large amount of inherent stability, but to leave considerable control in the hands of the pilot. I have suggested on previous occasions—and I believe these suggestions have been adopted with success—constructing the controls so that if released by the pilot they will fly automatically into the position giving maximum inherent stability. Thus, in case of the pilot failing, the machine would have a chance to look after itself.

In most of the oscillations to which a machine is liable the force tending to restore the machine to the normal position is small. If, therefore, the moment

of inertia is high and the damping effects are small, the oscillations may be large and persistent. It is therefore desirable to keep the moment of inertia within reasonable limits, and to provide suitable damping surfaces. Can Mr. Bairstow give us a table showing the moments of inertia about the vertical, longitudinal, and lateral axes through the centre of gravity of various well-known types of machines? In my experience, such tables are very useful. Could Mr. Bairstow also give us at the same time the damping couples on these machines for the various oscillations? As the science of design progresses, I believe that more and more attention will be paid to this question of damping out oscillations. The question is almost as important as the provision of suitable restoring couples in the machine, and it will probably be worth while providing the machines with auxiliary surfaces whose sole function will be to damp out oscillations. I am not sure from criticisms which Mr. Bairstow has raised in the past that he fully realises my views of this subject, but I have proved them to be correct years ago by constructing and flying models in a similar way to that now used by him.

It is not very clear to me why it is necessary that a machine should be "gravity controlled." Any such device would always take as its neutral line the line of relative force, and in the case of quick changes of speed or direction of motion, the line of relative force might conceivably deviate so far from the line of gravity that the conditions might be far from pleasant. There are three main systems of automatic control: (1) Aerodynamic, (2) Gyroscopic, and (3) Gravitational. The Wright automatic stabiliser belongs to the first type for longitudinal control. Will Mr. Bairstow be good enough to amplify his views on the subject?

Mr. F. H. Bramwell (*communicated*): During the discussion one or two points forced themselves very insistently on my notice, and it appeared that it might be of interest if these points—which are to a large extent fundamental—were brought out a little more clearly.

There are at present many men deeply interested in aeronautics generally, and in the question of stability particularly, who have not the time to give more than a perfunctory reading to a book such as Professor Bryan's "Stability in Aviation," and who thus do not become really familiar with the mathematical methods and equations he has used, which constitute the general method of attacking this type of problem.

Professor Bryan draws a very clear distinction between static and dynamic stability (page 187), and states very distinctly that it is not sufficient to have a machine which is only statically stable. An illustration from one of the models that was flown during the lecture will make this clearer. It is not sufficient for longitudinal stability that a machine has a pitching moment, which tends to return to its normal flying position if it is placed at a wrong inclination to its flight path. If it has such a righting moment it is statically stable, but not necessarily dynamically stable. This is shown very clearly by the model which showed an increasing phugoid; this model has a pitching moment tending to return it to its normal position, if it is placed at a wrong inclination; if it were otherwise, the model would have made a steadily steepening nose dive. As it was, the phugoid oscillation about the mean flying state increased in amplitude owing to the large moment of inertia of the model about a transverse axis. I trust I have now said enough to point out the danger of considering only the static stability of an aeroplane.

It seems necessary to mention the other point, lest anyone should be misled by Mr. Bairstow's remarks with regard to quickening up the phugoid oscillations without increased discomfort to the pilot. Those who have studied the question of either lateral or longitudinal stability in the way adopted by Professor Bryan

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will remember that each entails the solution of a biquadratic equation for "λ," where "λ" is the damping coefficient. Each of these equations will have four roots; and for the sake of illustration I will consider only the case of longitudinal stability. If a pair of these roots are imaginary, they indicate an oscillation, either damped or undamped. In some cases both pairs may be imaginary, and there are then two distinct oscillations. One of these pairs of roots is found to depend almost entirely on the wind forces on the aeroplane, that is, on its inclination to the relative wind; and in an aeroplane as ordinarily constructed this oscillation is either of very quick period and damped out with extraordinary rapidity, or more generally is aperiodic. It was this oscillation that was shown in the flight illustrating rapid oscillations. When the motion is aperiodic, the damping coefficient is generally such as to reduce the amplitude of the disturbance to one-half its original magnitude in about one-seventh of a second.

The other pair of roots depends very largely on the inclination of the flight path with respect to gravity; and the oscillation is usually slow (a period of about 20 secs.) and only slightly damped.

Mr. Bairstow's contention, with which I am in entire agreement, is that it is this oscillation which may be modified without causing more discomfort to the pilot, for the discomfort to the pilot caused by an oscillation increases with the quickness of the oscillation and the consequent rapidity of damping. Two methods of modifying this oscillation were suggested by Mr. Bairstow; firstly, by suitably altering the aerodynamic properties of the aeroplane, the period of this oscillation might be very much speeded up, with a consequent increase in the rate of damping, without making the period of the oscillation so quick or its damping so rapid as to give anything like the discomfort due to the other oscillation, which apparently does not at present cause excessive discomfort to the pilot, even in gusty weather. Secondly, these slow oscillations might be wiped out by the use of an automatic stability device, causing the operation of the controls. This automatic stability device must obviously be gravity controlled, as the oscillation is so largely dependent on the inclination of the flight path with respect to gravity.

I trust that these remarks may help to clear up one or two of what appear to be points of fundamental importance.

Mr. PIERSON also spoke.

Mr. BAIRSTOW: In the course of the discussion many very interesting points have been raised, and it is to be feared that the reply will be rather long. Many of the queries are a consequence of the condensed form of the original paper, the condensation being rendered necessary by the attempt to cover a large field in a short time.

It is proposed to deal with the discussion in the order in which the points were raised, and the first of these, by Mr. Booth, suggested that provision should be made in an aeroplane for balancing the propeller torque without attention to the lateral controls. This seems to be a case for the consideration of the arrangement of engine and propellers rather than of stability, and the desired condition can be achieved by using twin propellers. If in an aeroplane having only one propeller the engine is switched off, the motion of the aeroplane must inevitably be altered, as the result is to remove an external torque. In an inherently stable machine the motion produced by switching off would be a spiral glide, which might, of course, be checked by the pilot if desired.

Mr. Booth further remarked that Prof. Bryan's mathematical analysis does not apply to curvilinear flight. Strictly speaking this is correct, but in actual practice it is not necessary to wait for this extension of theory before proceeding

to make stable aeroplanes. The exact numerical details are much less important than the rules of procedure for experiment on a full scale. These are probably indicated with all the necessary detail by the theory as at present existent. It is probable, however, that in the immediate future the extension of theory to curvilinear flight will have attention from sources not connected with the N.P.L., but some mention is made in the Report of the Advisory Committee for Aëronautics, 1912-13, of the equations of motion in the general case. The differential equation is of the eighth order, and the conditions for stability are ten in number and except that there are more terms in the equation the difficulties are not different from those relating to rectilinear flight. The detailed solution has been deliberately set on one side at the N.P.L. whilst other matters tending to a better understanding of stability have been worked out.

Mr. Clarke, in expanding remarks in the paper, draws attention to the rapid upsetting of an aeroplane and its slow recovery, and correctly interprets my views in suggesting that the recovery might be hurried up without increasing the rate of disturbance. He then proceeds to refer to instruments operated by inertia, and in particular to the gyroscope, and refers to a remark in the paper that the gyroscope, unless gravity controlled, cannot be made the essential feature of an automatic stability device. The statement in the paper needs to be expanded somewhat in order to avoid misconception, for there is no physical reason why a gyroscope may not be a useful element in a stability device.

It is somewhat natural to suppose that an apparatus which moves the controls as a pilot would move them is an automatic stability device, and without discussing the pilot's mechanism to imagine that he is not gravity controlled. I feel quite sure that he is, and that the particular organs of importance are his eyes; it is probable that other organs assist his eyes, but the experience of pilots in losing their position when flying through clouds appears to me to be conclusive evidence that a pilot is gravity-controlled chiefly through his eyes. A gyroscope has no corresponding organ as an essential part of its mechanism. Mr. Schilowsky, the inventor of a new type of gyroscopically controlled monorail car, has recently pointed out most clearly the necessity for gravity control to produce satisfactory motion of an otherwise unstable car over the surface of the earth. I do not propose to discuss the difference in gyroscopic control as applied to stable or unstable aeroplanes, but the rules are now experimentally established by the work of Mr. Brennan and Mr. Schilowsky, etc., for monorails, and by Mr. Schlick for the control of the rolling motion of a ship.

Perhaps the best way of looking at the problem as relating to the aeroplane is this: An aeroplane is supported by air forces against the action of gravity. If it is to continue to fly steadily it must therefore keep its direction constant both relative to the air and to gravity, and no matter what type of stability is sought it must be such as to move the controls when the aeroplane is disturbed relative to either the air or gravity. If the stability is inherent, the gravity control is clearly present, although it is inextricably mixed up with the aerodynamic properties of an aeroplane. If an automatic device for an unstable machine is desired, the mechanism must supply the deficiency in the aeroplane and must be made sensitive to changes relative to gravity.

Perhaps the best field for the gyroscope is in applications which tend to reduce the upsetting and therefore leave less to be corrected by the inherent stability. I should prefer to call such an instrument an automatic control rather than an automatic stability device.

Mr. Handley Page's model of a stable machine which looped the loop was an exceedingly pretty example of the type. Some years ago Mr. Lanchester developed the theory of looping models and illustrated his theoretical conclusions by the flight of models. The important points needing attention in the production

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of a looping model are the provision of an appreciably large amount of inherent longitudinal stabililty and the launching of the model at a high speed as compared with its natural flight speed ; the lateral stability should not be too great, otherwise recovery may take place by side-slipping and diving. This latter is a very effective method of recovery, but tends to spoil the beauty of the loop.

I believe almost all advocates of inherent or automatic stability will agree with Major Brooke Popham that when an aeroplane is near the ground the pilot must be able easily to overpower the natural motion. It is easily realised that manœuvres near the ground will often require to be of a different character to those which the machine would make for itself. At the present time all that is asked for in the way of inherent stability is that an aeroplane shall not constantly be trying to upset itself, and if this be achieved, as it appears to have been at the Royal Aircraft Factory, the result is to make use of the controls less frequent without reducing their powerfulness.

Mr. Busk has for some time now been making full scale experiments on stability. I believe that he will agree with me in saying that the results fully support the mathematical analysis. The application of method to the production of stability does not apparently involve any unforeseen factors, and a stable machine has now been built as a consequence of the application of mathematical analysis, and the results of experiments on models, combined with the skill of a pilot in interpreting the conclusions in actual flight. By the time these remarks are made public the final tests of Mr. Busk's experiments on the stability of R.A.F. aeroplanes will be well known.

Mr. O'Gorman referred to the undesirable character of a large vertical fin at the instant of alighting in a side wind. It may, perhaps, be well to point out that the real fin corresponding with mathematical analysis, or the fins on the models illustrating that analysis, need not be so apparent or so troublesome. In his book on stability Prof. Bryan showed that a dihedral angle on the wings of an aeroplane was equivalent to a very large vertical fin above the body. In practice, therefore, a dihedral angle is the best way of obtaining a fin above the centre of gravity and will in all probability be the method adopted for assisting in the production of lateral stability.

I cannot do better than state my entire agreement with the views of Mr. B. Melvill Jones, who was, until recently, one of my valued colleagues at the N.P.L., and who shares the responsibility with Mr. Thompson and myself for a report on stability in the Report of the Advisory Committee for Aëronautics, 1912-13.

Colonel Fullerton raises the question of the importance of angular or rotary derivations in stability, and suggests that they may safely be ignored. To a certain extent I am in agreement with his conclusion, but only so long as it is applied to longitudinal stability. In the case of lateral stability rotary derivatives are extremely important and should not be ignored.

The effect of change of loading on stability is felt in two ways, either as a change of attitude or a change of speed. There are no simple rules relating the stability to the loading.

In the near future it is probable that more attention will be paid to body form in relation to stability. At present the difficulty is lack of information. Efforts are being made to provide the necessary experimental data.

Mr. Thurston suggests that it is desirable to keep moments of inertia of an aeroplane small. There is another point of view, however, for large moments of inertia mean slow periods, and rapid variations in the wind are then less important. The ultimate solution will probably be a compromise to obtain both stability and steadiness of flight.

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Mr. Bramwell has called attention to Prof. Bryan's distinction between static and dynamic stability. In certain cases the change in disposition of the surfaces of the machine to convert from one to the other is small, but there appears to be just one danger in speaking of "static" stability, *i.e.*, in the production of righting moments for any displacement of the machine from its steady flying position. From common usage "static" implies the forces on a body when "stationary" and it might be taken as implying that it is sufficient to have righting couples on an aeroplane held stationary in a wind in any position. Such righting moments, although necessary, might not produce stability, and it seems to be important to consider, in addition, certain rotary coefficients, particularly those which refer to the lifting of the outer wing when turning, and the turning of the machine which accompanies rolling.

The CHAIRMAN, in proposing a vote of thanks to the lecturer, referred to the faculty Mr. Bairstow possessed of expressing abstruse problems with great lucidity. (Applause.)

Mr. MERVYN O'GORMAN said that the thanks of the Society were due to Sir Alfred Keogh for his kindness in presiding that evening, and he had pleasure in proposing that a very hearty vote of thanks be accorded to him. The motion was carried with applause, and the meeting terminated.

THE SIXTH MEETING of the FORTY-NINTH SESSION of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 4th February, 1914, at 8.30 p.m. The Right Honourable Lord Sydenham presided.

FURTHER DEVELOPMENTS OF MILITARY AVIATION.

BY LIEUT.-COLONEL F. H. SYKES, R.F.C., ASSOCIATE FELLOW.

Introduction.

MY LORD, LADIES AND GENTLEMEN,

Last year the Aëronautical Society kindly asked me to give my views on Military Aviation. I then tried to show the objects for which military aircraft would be used, the results likely to be gained, and the general lines upon which it was expected that progress would take place—in a word, the military aspect of aviation. Airships have now been handed over to the Naval Wing of the Royal Flying Corps and I propose to-night to deal almost entirely with aeroplane work, in the light of a year's progress. I make no excuse for reiterating the strong links which I feel bind soldier, sailor, designer and aircraft constructor in this matter. Rapidity of progress is essential and it can only be gained by cohesion of effort, lack of friction, and mutual understanding. The Service must remember that the aeroplane designer has to live and is not always merely chasing "X" with a slide rule; the constructor, that the soldier is not only pipe-clay and red tape and that there is sometimes method in his madness.

The object of military aviation is the unselfish service of the State. It is essential that agreement should be promoted as to general lines of work needed and advisable progress as to types of aircraft possible, and how they are to be built and used.

In this connection we must remember that in the Services "the best is often the enemy of the good." We cannot only look for what we want in the future. We must have a very tangible something ready for war in the present.

I hope, at all events, that this paper may afford grounds for a discussion which will be useful to us all.

It will touch as little as possible on matters dealt with in last year's lecture. The progress made during 1913 tends to corroborate the ideas then put forward. Progress in aviation has not affected the principles of war strategy, nor will it, in my opinion, do so in the future, but strategic plans will require to be even more carefully worked out in peace than hitherto. An all round acceleration will take place and less confusion occur. The cards will be more openly displayed.

Various "milestones" have been left behind. The past year has been one of great interest. Safety, speed, strength, weight-carrying powers, climbing, and all-round efficiency have progressed. Some probabilities have been increased; some difficulties emphasised.

The burden of our poor General Brown-Jones of last year has indirectly been lightened. But directly, though the height record now stands at over 20,000 feet, the strategic problem of broken roads, railways and bridges, remains unaffected. Though an endurance record of nearly 13,000 miles in 39 consecutive days was carried out last year, weary, hungry, pack-carrying infantry are not materially

assisted in their efforts to foot-slog an inch further through heavy mud or dust. Though Védrines has "done" Egypt by air, though Mr. Orville Wright now "considers the trans-Atlantic flight a possibility," the strain on Generals and Staff is much as it was. Even the most brilliant gyrations of "loopers" leave Brown-Jones cold when grappling with how to beat the enemy.

The results achieved by the new instrument of war are nevertheless of ever-growing significance and a short survey of the more important points may be instructive.

I propose *first* to deal with the improvements arrived at and indicate the more pressing requirements; *secondly*, to turn to matters more directly appertaining to observation and then to execute what in journalese is, I believe, called a risky vol-plane to landing places and ground organisation through the hazardous "remous" of intercommunication and delivery of information. A flight to demonstrate the uses of aircraft and their effect in military operations will then follow, and the display will terminate with a daring series of loops and tail-slides spelling past, present and future.

Wind Flying and Duration.

The first improvement is undoubtedly that of general airworthiness. Last year I said that the use of aeroplanes was limited by fog, darkness and high winds. One of these limitations—wind—is already fast disappearing. A most marked advance has been made in long day-in day-out flying, almost irrespective of weather. I remember my delight on the evening of my lecture to this Society last year on receiving a telegram to the effect that the five aeroplanes, which then went to form the nucleus of our second squadron, had all safely reached their new station at Montrose. The flight from Farnborough is a fine one to-day, but a year ago it was a splendid flight, more especially as the weather conditions were bad. It took about a fortnight. Lately several of our aeroplanes have effected the same journey, under almost equally adverse wind conditions, in two days, and it has twice been done in good weather, in one day (once without a stop of any sort).

There are other indications of progress in this respect; for instance, before and after the Irish manœuvres our aircraft had to move some four hundred miles out to the manœuvre area and the same back (including two crossings of the Irish Sea), in addition to the actual work carried out while there on manœuvres. This was done without serious hitch.

Factors of Safety.

I am not quite certain that sufficient attention has been paid by designers to the fact that flying in high winds has been very much on the increase during the year and that factors of safety have therefore decreased. This is the more so in view of the fact that such strides are being made in the direction of variable speed.

Climbing and Lifting.

Another marked improvement is in the climbing ability of the average aeroplane. A service machine *must* be able to lift and climb well with a full load in windy weather, and four hundred feet a minute is now quite an ordinary achievement.

The notable advances made in variable speed, as exemplified for instance in the latest Sopwith, have helped greatly in the matter of landing in restricted areas.

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Wing Warping.

One cannot consider airworthiness without touching on the question of wing-warping as opposed to flaps. There is no doubt that the continual flicking about during a long flight of the control lever, caused by the self-warping of wings in a wind, has a very tiring effect on the pilot. Further the warping wing requires more keeping in truth than one fitted with flaps. The dismantling and general handling of wings fitted with flaps is, besides, easier, quicker, and less liable to mistake. There is, I think rightly, a growing tendency to substitute flaps for warping.

Improvements in Landing Gear.

Bad landings must be expected from aeroplanes which are often called upon to bring back information to, and land in, a small field, probably surrounded by trees. Landing gears have considerably improved, but they still cannot withstand really rough treatment.

Such have been some of the advances as regards material and design. Progress in these and in other matters is still required. A little too much trust in Providence is still asked for.

Military aviation, both in peace and in war, requires the utmost possible simplicity and strength. Much more ease is necessary in the removal and return of all parts. If an aeroplane is flying morning and evening it has at present still to be gone over more carefully during the day by its mechanics than should be necessary, and every fourth day it has to be left in for thorough overhaul and trueing up.

To and from and during the actual days of manœuvres of last year our aeroplanes flew an average of approximately 800 miles. The number of hours flying per day that can be expected from pilots is going up slowly and steadily. This is due to the better training of personnel, to increasing reliability of machines, to improved arrangements for transport and spare parts—in a word, to better all-round training and organisation. Last year I said that pilots and observers could only be employed for about three hours during the day. Now, in good weather, for a short period, five hours flying a day can be done by good and seasoned officers.

The Motor.

Broadly speaking, the question of duration is now largely bound up with that of obtaining—

(*a*) A good motor—including high fuel efficiency.
(*b*) One capable of carrying the increased weight of fuel.

Engines are improving, but not very rapidly, though cases of failure are undoubtedly becoming more rare. A really reliable good engine of sufficient power is much wanted. This remark applies equally to British and foreign makes. I would add that a very intricate engine, whatever "star" performances it may carry out under the eye of its maker and his pet mechanic, is not much use for the every day purposes for which we want it.

Two engines may perhaps be the solution of many difficulties, but each still requires to be of a more reliable standard than is at present the case.

I think that an opportunity is given by the forthcoming Competition.

Great progress has been made in the ability to keep the air in all weather for prolonged periods and to land in and rise from small areas, but I agree with

the general view that there is still room for much improvement in the simplicity, strength and reliability of both aeroplanes and their engines.

NOTES ON OBSERVATION.

In regard to observation. As a result of work done during manœuvres, it would seem that given sufficient aircraft, failure to locate the enemy's movements may be due to two causes:—*First*, failure to carry out the reconnaissance owing to bad weather, engine trouble, failing light, etc., and *second*, failure owing to bad training or lack of skill in observation, to measures taken by the enemy to conceal himself, or again, weather.

None can control the weather, and engine failure is not altogether preventable. Knowledge, experience, training, and very careful selection of observers, and plenty of practice for them are amongst the obvious remedies.

The instructions issued in September, 1910, for the manœuvres of that year state, "it is possible that our aeronauts may not readily find their way across country, and it must be borne in mind that they have had no practice in observing military operations from above."

It is quite possible that some aeronauts (even in this room there may be some) do not always readily find their way. But, as a matter of fact, there is no abstruse question of navigation about it, as one so often hears, it is mainly one of eye for country, aptitude for reconnaissance, and practice (for which, by the bye, many machines, preferably of the propeller type, are necessary).

A sound military knowledge is also essential. It is so pleasant in peace to go up for a "joy ride"; to feel the strong yet delicate machine tear one gently into the rushing clear air; it is pure exhilaration to watch the colours of the earth dim into greyness and gradually into purplish haze. But go up as an observer in war, your brain slow and tired after many days and nights of strain. Go up well knowing the fallibility of your machine. Go up feeling that your flyer is weary, that your machine and engine are tired. Know that much, very much depends on you, your vision, decision, accuracy and certainty. You see something. Your flyer throttles down to the lowest safe flying speed. Unless your faculties are well trained, you have passed it and have not decided what it is.

Training of Observers.

Long training and much practice for observers is essential. And, as I have said, good observation can only be carried out satisfactorily, on the basis of a large and sound knowledge of military matters.

In working in the area traversed by a large river, considerable difficulty is often experienced owing to mist and fog. Where there are mountains, there is likely to be rain. It is indeed probable that the climate of the theatre of war will, one way and another, much affect the amount of air reconnaissance possible. Perhaps even, on occasions, columns may take advantage of lines of advance known to be more than usually mist-covered. Such areas should be noted beforehand, both by troops and by aircraft.

When fighting takes place in hilly country, observation is often difficult, owing to the tops of the hills frequently being under mist or clouds. In this and other such cases the rule that observation can only be made at heights of over 3,000 feet must be followed in the spirit rather than in the letter, otherwise observers may "be in the clouds" the whole time and see absolutely nothing.

Heights flown during reconnaissance must, then, be governed largely by the state of the atmosphere, and the consequent ease of observation. Questions of

immunity from bullets must not affect this. Such immunity as we have in immediate sight is quite promising. If the flyer, observer and engine are placed under armour they are safe from rifle fire at, say, 3,500 feet. As regards spars there is, I think, really not very much to fear. The factor of safety is good, as it is only in the unlikely event of two bullets piercing the spar in immediate proximity, and at right angles to its length, that serious danger may be apprehended. With reference to wires, it is necessary not merely that two separate wires should be cut but that they should be interdependent. The only other vulnerable spot as far as I can see is the propeller boss.

It was thought desirable in the interests of training to elaborate in detail the results of reconnaissances carried out during last manœuvres. We had also the object in view of showing the army at large what can and cannot be effected at this early stage. The results show what there actually was on the ground, and what was reported as being seen from the air at short intervals, throughout the whole operations. At first it seemed that we had located nearly everything, then as the work grew, it appeared that a certain amount had been missed, but the final result showed that really very little had been missed. I think, on the whole, the information obtained was good.

The elaborate precautions taken by certain bodies of troops, notably the 4th Division, were responsible for some of the failures.

Concealment.

With reference to efforts in concealment made by troops—as a rule those made during a halt on the road are not very successful. Broadly speaking, two methods of concealment are adopted. One consists in taking cover whenever an aero-engine is heard. The other in elaborate arrangements in bivouac. The first method is, on the whole, not a very successful one, owing to the fact that there is always an uneasy look about the road which attracts the attention of a good observer—the tail of a wagon, a deserted cooker, a belated ambulance, gives the show away. Transport and guns disguised as travelling shrubberies are not generally difficult to spot on the march. The second plan, however, meets with undeniable success, and further experiments in this direction will undoubtedly be made.

Photography.

Photography is a most useful auxiliary to the observer, who must, however, not use it to save himself. He must observe just as accurately and intently as if he had no camera. Photography is difficult from an aeroplane owing to the fact that the observer is often rather cramped and the camera has to be fixed in the bottom of the machine. The observer, too, has so much to do and so many things to look after—his map, field glasses, duplicating report book, message bags, signal lights, etc., etc., and he may get a shower bath of petrol if he does not look after the supply in the gravity tank! In my opinion there are considerable possibilities in the science of aerial photography, and it is well worth developing.

Night Work.

A word may not be out of place now with regard to night work. Here the great difficulty is that aviation motors, as I have said, are still far from being really reliable, and, as under-carriages are not very strong, an enforced and hurried landing entails considerable danger.

The lights carried on aeroplanes for flying by night across country are still in an experimental stage. The problems are those of weight, safety as regards

fire, lack of glare and good diffusion of light, and range of beam. One way and another, though progress is being made, there is considerable risk attached to night work, especially in England.

To sum up the question of observation, although a very solid all-round advance has been made in day work, night work, in so far as the aeroplane is concerned, has shown no tangible results. Night work at present lies in the scope for the aeroplane which can, for certain, fly for 14 hours with two engines and fuel for this endurance.

INTERCOMMUNICATION.

Having observed, the observer has now to communicate his information by the most rapid and safe method possible. The question of the best means of intercommunication is a difficult one. It involves problems of both materiel and personnel, and considerable time and experience will be necessary for their solution on sound lines. When we have solved them we shall have minimised the relative importance of many other difficulties, such as questions relating to landing grounds, alighting in small areas, ground communications, and under-carriages.

Wireless.

Wireless will undoubtedly be one of the best methods of transmitting from aircraft the information collected in reconnaissance. It was used with success by the army airships on last year's manœuvres, and the results gained were, I believe, in advance of anything which has hitherto been done either in England or abroad. In hilly or very closed country wireless transmission is essential. But the whole question of wireless in connection with aircraft is a thorny one. There are, for instance, the well-known difficulties in regard to tapping and jamming. Close study and much experiment is necessary. The prospects of using cipher for wireless messages are hopeful, though messages regarding reconnaissance call for quick transmission when sent from a craft moving at 70 miles an hour, whilst the ordinary process of enciphering is slow. A solution has been hit upon which has worked well, but so far it has only been tried with a small number of messages.

Message Bags.

On occasion great responsibility must rest upon the observer. He must sometimes decide instantly whether it is desirable to transmit his information direct to the troops most concerned or to the staff. It will often be advisable to drop a message bag with some urgent piece of information in order that the news may be received at once. It requires considerable practice to drop messages in this way, and from some types of aeroplane it is dangerous to do so owing to the possibility of a bag being caught by the propeller. The bags also sometimes do not reach their destination.

On the whole then intercommunication still presents many difficulties, and the present more generally adopted system of landing for the transmission of information on the ground must be adhered to for a time.

HANDING IN INFORMATION.

Landing Grounds.

It has often been found a matter of much difficulty to find landing grounds in the vicinity of Headquarters. A great deal depends on this, as a distant landing ground probably entails loss of very valuable time.

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During manœuvres an umpire was attached to each squadron, whose duty it was to examine each observer's report handed in at the landing ground. He checked the barograph record before allowing the report to be transmitted in order to insure that the flight had taken place at such a height as would probably have enabled the information to be safely brought in.

All experience has tended to emphasise the necessity of having the landing ground very close to headquarters. In order to ensure this the best plan is for the officers responsible for ground arrangements to survey the area from the air. The most likely fields are more easily spotted in this way than by any amount of reconnaissance by road.

The larger the field the better, but in this connection it is interesting to note that it has been found that some fields which are just too small to be of use as landing grounds for 70 H.P. Renault B.E.'s, are just large enough for M. Farman's. For this reason it is quite possible that in some types of country information obtained by an observer in a M. Farman, will be in the hands of the General more quickly than that obtained in a very much faster craft. In windy weather this does not obtain, for obvious reasons.

For peace training work it is, I think, a good suggestion that all garrisons should look through their areas with a view to recommending one or several good landing grounds in their vicinity. Mayors of towns might also be asked to do the same. Local football grounds would in many cases be suitable, and an undertaking would, of course, be given that the grounds would not be used by the R.F.C. on Saturdays or when otherwise required. Here is a golden opportunity for Mayors! A Royal Flying Corps officer would then be sent round to see if the grounds were really suitable and draw up a rough report for inclusion in an air-route handbook which could then be compiled. We have already collected much data for such a book.

There are many areas in the British Isles which demand very slow-landing machines if constant mishaps are not to be the rule. Possibly air or ground brakes may overcome this difficulty. The method of altering the angle of incidence of the wings while the machine is in flight does not appear to be a sound solution.

Landing grounds should preferably be selected well away from sheets of water or low-lying ground, as it may be found that morning and evening reconnaissance work will be much impeded by mist when the surrounding country is comparatively clear.

To sum up, the bulk of information will be brought in to landing grounds. Their selection is therefore a matter of great importance, and necessitates a considerable knowledge, not only of the requirements of aircraft, but also of the conditions affecting the transmission of information to the Staff.

This brings us to the very important question of ground organisation.

GROUND ORGANISATION.

Personnel.

Added experience only goes to emphasise the necessity for a high degree of military training and technical efficiency in the ground personnel, not only of those responsible for attending to the machines, but those employed on other duties such as transport, intercommunication, etc. Success in war will depend as much on the efficiency and keenness of the ground personnel as upon those whose duties are more essentially in the air. And we should not overlook the fact that, although our officers and men are of the keenest, those employed on ground work must be continually subjected to the disheartening effects of seeing the results of their hours of toil brought to nothing by the mischance of a second.

To the good air mechanic the machine for which he is responsible is his pride. I know men who regard their machine almost as a living thing. They are intensely proud of its achievements.

We are most fortunate in the fact that our officers and men are splendid; and I may say that such shocks as I have mentioned have no effect on their keenness. Their efficiency is indeed increasing more rapidly than we have had a right to expect in view of the limited facilities so far obtained for training.

The rapid expansion of the Corps with the consequent dissemination of the old hands has also militated against the immediate development on solid lines of our ground organisation, though it is steadily improving with experience.

I would like to add here a plea to manufacturers that they should look with sympathy on the Military Wing of the Royal Flying Corps, and co-operate with it in connection with the employment of good men when they leave the Corps. In two or three years' time some men will be leaving the Service. They will prove—I say it advisedly—to be possessed of a high standard of character and technical ability and will be available for and anxious to obtain skilled employment in aircraft factories. I will go further and ask employers not to throw obstacles in the way of men wishing to join the Military Wing or its Special Reserve.

A point brought into relief is that a large number of officers is required, under present conditions, to cope with the work. They are wanted for piloting, observing, gunnery and command and administrative duties. Hitherto piloting has had a tendency to obscure the great importance of the remainder.

Material.

I think it is often forgotten by soldiers and constructors in connection with casualties of material that we are apt to base our experience entirely on manœuvres. These generally only last a few days, and I think it should be remembered that the repairs and replacements which become necessary in the field do not give a fair indication of the average amount of such casualties to be expected in a longer period of campaigning. Much of the material is new at the commencement of manœuvres and does not become unserviceable in so short a period, whereas in a longer period parts would deteriorate sufficiently to require renewal in the field. A reliable basis for requirements in this direction is very difficult to obtain. At present we can only guess at the figures.

Sheds.

Some aeroplanes deteriorate more rapidly than others by being out in the open, but there is no doubt that they all deteriorate to a very considerable extent, and if the climate is damp or otherwise unfavourable, new planes will be required after each period of manœuvres. More attention to varnishing the wood and exposed metal would probably well repay time given. A really good waterproofing "dope" is a want.

An entirely efficient very light portable tent is still lacking. If the ideal in this respect is obtained, considerable expense and deterioration of aircraft might be obviated by at once sending the tent shed to the breakdown, instead of having to go through the laborious process of dismantling the broken-down machine, loading it on a lorry and bringing it in.

Transport and Accessories.

Our designs in the matter of transport are gradually hardening and are tending towards standardisation and simplicity.

The amount of transport and accessories required in the field is at present large. But this must be in inverse ratio to the reliability and simplicity of aircraft

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and to the degree of training of the personnel. We are now, I think, arriving at a sound organisation for the maintenance of aircraft in such particulars as petrol, oil, spare parts, repairs and supplies. Our great aim, however, is to make the maintenance of aircraft as simple as that of the field gun.

In this, as in all matters relating to the Flying Corps, time is required to enable the internal organisation to bear fruit.

USES OF AIRCRAFT.

Having dealt with matters of detail as they at present exist, I will now try to formulate ideas as to the employment of aircraft in war.

Experience places the value of aerial reconnaissance beyond a doubt, not only in calm weather, but practically in all weathers. Also it has been proved that there is no inherent impossibility in maintaining aircraft in the field.

An examination of the estimates of the various Powers under this head shows how clearly the fact is recognised.

Aerial reconnaissance itself is much; it will however be opposed, and that in the air. Anti-aircraft guns will assist, but probably insufficiently.

This fact being granted, there is the obvious necessity for different types of aircraft. They will undoubtedly be required to perform several distinct duties, and thus the question of the most suitable composition and distribution of flying squadrons to different portions of an Army, is clearly of much importance, both to soldiers and to aeroplane designers.

Does the advanced cavalry require one type of craft? The Headquarters of an army another? Flanking divisions a third? Will there be a battle squadron? A fast scout flotilla? A squadron to hunt down and destroy airships and to attack aircraft bases? A low-flying armoured destroyer of ammunition parks and supply trains? A heavy transport convoy craft? A breakdown and repair craft?

For all these duties, slightly different types and qualities are required. Even now, one can hardly imagine the tabloid Sopwith taking the role of a Sikorski argosy!

STANDARDISATION.

Whatever the future may bring forth, I would like to emphasise as a principle, the necessity for all possible standardisation in the reproduction of any one design. We must weigh most carefully the merits of any new design, but at the same time, check any unsound diversity. Standardisation of types is at present undesirable, but standardisation within types is essential. Every new pattern and every fresh modification spells increased stores, increased transport, and therefore loss of readiness and mobility of the force as a whole.

Mobility, and readiness for instant action at any time, must be the essence of being of a Flying Corps.

A Flying Corps which is not in the field when the flag falls, if not before, has lost a large proportion of its utility.

But to return to types.

I do not suggest that a rapidly developing science like aviation may not necessitate the unhesitating abandonment of one design for another, but the guiding principle must in any case be that of the fewest possible types compatible with the object to be attained. With each new type, innumerable attendant difficulties of maintenance, replacement, and storage are forced upon one. This

question is one of less importance in the case of the Navy, as it has no difficulties of land transport, but it is vital to an army.

AERIAL FIGHTING.

Hitherto, progress in military aircraft has had to do mainly with reconnaissance machines, and these have already been dealt with at considerable length, but the fighting aeroplane now claims attention.

The problems raised or inherent in the question of aerial warfare are very complex. Command of the air will undoubtedly be sought. It will, as undoubtedly, be difficult to obtain. The third dimension—climbing—remains one of the principal stumbling blocks.

There are two schools of thought regarding fighting in the air. The one holds that if an aeroplane is to fight, it must carry a passenger, gun and ammunition. It will be so large and heavy that it will be slow, also it will lack the means of intercommunication necessary for combined action, and it will be unable to come within range of a fast scout. The latter will come, get its information, and go, unmolested. It would appear that, for a time at all events, the fast scout will have the advantage. This depends largely on the number of fighting machines available.

The other view is that fighting in the air must occur if results are to be obtained. Given that one side has sufficient fighting machines, it should be impossible for the unarmed scout to approach the point where it desires to glean information.

We have as yet nothing to go on in this matter. It is, however, of great importance that thought should be directed, by all concerned, to the problem, and to that of action against troops on the ground. Aerial tactics will take much working out, and an aerial building policy can only properly be based upon them.

Identification of Aircraft.

A first requisite is the recognition of friend and foe. No system has yet been evolved to ensure the distinction and recognition of aircraft either from the air or from the ground. Instances do and will occur of rifle and gun fire being directed at aircraft, but it seems generally that troops refrain from firing owing to the impossibility of recognising friendly from hostile aeroplanes, and from fear of firing at friendly ones.

Reliance cannot as yet be placed on recognising hostile aircraft by their type. Moreover, as the speed of aeroplanes increases the distances at which they must be identified in order to stand any chance of hitting them also increases.

Black and white stripes painted on the under surface of the planes are distinguishable at two or three miles, according to the light, but an aeroplane cannot be recognised at any considerable distance. It is as difficult as it is important that a satisfactory method should be found, in order that it may be possible to issue orders to the troops to fire at any aeroplane, not definitely identified by some means as friendly. Various means have been suggested, and some attempted for ensuring this, but so far with no marked success.

SUMMARY OF POINTS BROUGHT OUT, REQUIREMENTS AND SUGGESTIONS FOR CONSTRUCTORS.

I will now endeavour to summarise the points which I have tried to indicate during the course of this lecture.

First: I would mention the paramount necessity for a really reliable, suitable, high-powered and silent engine.

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The **second** is that of factors of safety under the conditions of present aerial flight.

Third: The apparent advisability of flaps as opposed to warping.

Fourth: A better landing gear to sustain the shock of landings such as must often be made in a Service machine.

Fifth: Progress required in the directions of strength and simplicity in design and construction, so as to reduce the time and labour now required to keep aeroplanes and engines fit for constant service.

Sixth: Time would be usefully expended on experiment for better methods of intercommunication between aeroplane and aeroplane, and aeroplane and the ground.

Seventh: Standardisation. There is method in the military insistence on limiting types to those absolutely essential and in requesting a rigorous standardisation of parts.

Eighth: A large type of machine with, if necessary, multiple engines and propellers, is now required.

Ninth: Invisibility.

Tenth: Air or ground brakes.

CONCLUSION.

In conclusion, from the military point of view, I am glad to think that the words Sir John French used, when kindly presiding at my lecture last year, have been to a certain extent taken to heart. He said, "Aviation is one of the most important subjects to which the modern officer can pay attention at the present day." Interest in and knowledge of the subject by both Services have increased, and public apathy has shown occasional signs of movement.

Aviation has advanced and is still advancing at an unparalleled rate. May I quote to you the leading paragraph of the instructions issued in September, 1910, for the manœuvres of that year. They already read like Chaucer: "As this is the first occasion on which aeroplanes and dirigible balloons have been employed in this country for military work, their employment is largely a matter of experiment; and as the science of aerial navigation is yet in its infancy, unreal conditions which would not obtain in war, must be observed for the safety of the aeronauts and their machines."

This, I think, constitutes a landmark. It is difficult to realise that these instructions were issued less than three and a half years ago. Where there were a very few isolated and groggy machines quite unsupported by ground organisation, there are now squadrons. This progress is producing new strategical considerations affecting radically all military arrangements and dispositions.

The aerial scout is well evolved.

The "fog" of war is already turning to mere mist.

Now the fighting machine which will eventually affect the destinies of nations is materialising.

Very soon we shall have aeroplanes with considerable weight-carrying capabilities, with a speed of, say, 100 miles an hour, and a capacity of 700 miles. They may be fitted with transparent wings. With such machines the flanks of even modern armies will be easily turned, vital arteries of supply, well in their rear, easily gained, the words "overland" and "oversea" will have little meaning.

It appeals very much to the imagination to think of a squadron of almost invisible and silent aeroplanes launched to raid some vital spot of the enemy's organisation.

A similar squadron if armoured might perhaps safely fly low over parked ammunition and supply-wagons and sow explosives amongst them.

I do not agree that these ideas are the product of too vivid an imagination. We now have the ability to keep the air in most weathers. Great weight-lifting aeroplanes are an accomplished fact. I would therefore urge British manufacturers to develop their designs for large machines; there can be no doubt that such machines will be required in war. I am aware that however good the will, bricks cannot be made without straw. Nevertheless, a year or even a few months' advantage *now*, may mean *much* later.

This country is certainly not yet first in aerial strength, but the year's effort has been productive of such tangible progress as to show that we could attain aerial supremacy if we really meant to. *Do we mean to?* Believing as I do that land fighting and sea fighting will eventually wait on aerial fighting, I am convinced that we should make every possible effort to attain that end; and this effort should be made at once. We cannot afford to delay.

DISCUSSION.

Mr. W. Joynson Hicks, M.P., asked for information as to the future organisation of the Air Service, and went on to formulate certain views as to the relation of aeroplanes to other scouting units. He considered that aeroplanes should be under the command of Generals of Divisions, as in that way better use could be made of them than if they were all attached to the Headquarters Staff. He wished to be informed as to the intentions with regard to the relation of the Royal Flying Corps to the Navy and Army Services. He dissented from the proposed extension of the number of types of aeroplanes, and asked whether a few types could not do all that was required.

Captain Waterlow, in continuing the discussion, said that at the outset the lecturer had expressed his intention of referring to the progress made during the past year in military aviation. As was well known now, the military airships had been transferred within the past six weeks to the Admiralty, and he had therefore hoped that at all events this would have been the opportunity to mention the work they had done. The military airships had achieved a good deal during the past year. On manœuvres one of them was out, and sent back useful observations on two occasions when mist and low clouds rendered the aeroplanes useless. Wireless from the airships had been very successful; more so, in his opinion, than in foreign countries. It had been found possible to transmit from one airship to another while in the air thirty miles apart, and to receive messages thus sent without stopping the engines. Wireless messages sent back some sixty miles to the receiving station had been received, decoded, passed the censor, telephoned on to Army Headquarters, and actually read by the General within seven minutes of the time of transmission.

He would therefore urge them to remember that military aviation included airships as well as aeroplanes.

Brigadier-General D. Henderson, in reply to a request from the Chair, expressed his views on certain points arising out of the paper and the discussion. He agreed generally with Colonel Sykes, although he had his own ideas as to possible future developments. He deprecated any dogmatic attempts to lay down hard and fast rules as to the relation between aeroplanes, the Headquarters Staff, and divisions. He certainly thought, however, that in the initial stages of the

battle, when divisions were very close together and the enemy far away, it would not be at all practical to have the aeroplanes attached to divisions, as the result would be that they would have possibly several machines setting out from bases a few miles apart to reconnoitre over the same hundred miles, say, in the direction in which the enemy lay. He thought, therefore, that the aeroplanes should be directly under the hand of the Commander-in-Chief, except possibly in cases where, when the opposing armies were close together, a few aeroplane squadrons or flights might be detached to operate under Divisional Commanders for the time being. The best thing to do was to wait and see what happened in war, and act accordingly.

Mr. Joynson-Hicks had mentioned the value of the fast scouting aeroplane as against the slower fighting machine. There were limits, however, to the possible speed of the former at present, owing to the necessity for landing, in order to hand in information. Speed alone, without a range of speed, is not sufficient, and a scouting aeroplane of the type of the Deperdussin flown by Prevost in the Gordon-Bennett Race, which ran for nearly a mile after landing, would make it doubtful if any use could be made of the information obtained by its pilot, if he tried to land in this country. Although he was not prepared to admit that the fastest aeroplane was going to give success in war to the side that owned it, if they wanted to know which side had the fastest aeroplanes in its military organisation, it was this side at present, and would be for some time.

Mr. G. Holt Thomas referred to the enormous progress of military aeronautics in this country during the past year or so. What he would like to testify to was that, from personal experience, he thought the British Army pilots as a body were far superior to anything the Continent could produce. Abroad, of course, they could put up very fine individual fliers, but for real all-round work our men were easily first. He looked forward to our progressing until we had the best Flying Corps in the world. He certainly thought we were fortunate in possessing the men.

Major Baden-Powell: After hearing the most interesting remarks from such authorities as Colonel Sykes and General Henderson, you will not want to hear much from me. I would, however, like to comment on one or two points which have been touched upon. Some speakers have referred to the number of men which airships or aeroplanes are capable of carrying. It has been said that, because one type of aircraft can carry twenty men, whereas another can only take two, therefore the former must be superior for war purposes. But what is the advantage of so large a crew? One man can see as much as a dozen, so for observation we only want one, or at most two. One can steer the vessel, with possibly a spare pilot to relieve him on long journeys, so that a large number of men is quite unnecessary when the craft is used only for observation. On the other hand, looking to the distant future, when we possess hundreds of machines, each capable of taking ten or a dozen men for a long distance, then we get to the question of being able to go over the enemy's country, even during an engagement, and dump down a force of a thousand men on any given spot where they may do very effective service, and later, when the situation is becoming a bit warm, they may re-embark and fly back to their base. This seems a subject well worth bearing in mind.

The Chairman: Before calling upon the lecturer to reply, perhaps you will permit me to make a few remarks.

Colonel Sykes has covered a very large amount of ground, and has handled his subject with great ability. He has shown the remarkable advance made during the last year, and has indicated the direction in which progress is moving. He has thrown what is to me a new light upon some important aspects of aeronautics. He has given us all much to think over, and he has suggested a golden opportunity

for Mayors in connection with landing places, of which I hope they will hasten to avail themselves.

To me the probable effect of aviation upon war by sea is an intensely interesting subject. Considering what has been already accomplished, it would be rash to set limits to the developments of the future, and what I am going to say may be falsified by the further advances which we may expect.

At the present time the aeroplane appears to me to be more important than the airship, which, however, has certain advantages already, and may have others in the future :

The power of remaining longer periods in the air, allowing the crew to take rest.

The ability to poise in order to make deliberate observations.

Greater facilities for navigation in some conditions.

These advantages are great, as Captain Waterlow has pointed out so clearly. On the other hand, there are certain disabilities in the airship which have already caused disasters, and which in actual war may be accentuated. By day, I do not think an airship could live under artillery fire at 5,000 yards, or even more. An airship attacked by three or four aeroplanes would, so far as I can see, find herself in the greatest difficulties. The greater speed, the superior manœuvring power, especially in a vertical plane—400 feet rise in a minute, as Colonel Sykes has told us—of the smaller and far cheaper weapon, must tell heavily against the relatively cumbrous craft. There will be great advances, as Captain Waterlow has foreshadowed, and I am most strongly of opinion that we must not neglect airships, but should experiment fully with them and find out all their capabilities. Meanwhile, however, I am inclined to believe that our policy should be to prepare to attack them.

I may be quite wrong, but I do not at present see any great military possibilities in dropping bombs from airships or aeroplanes. But I am well aware that some local effect was thus obtained in Tripoli and in Turkey. One can conceive that the stampeding of a large number of animals might in some circumstances have important results, and the moral effect in Indian Frontier warfare might be considerable.

We are all agreed that for purposes of reconnaissance the aviator will be of immense value. So great is that value that it has often been suggested that, before undertaking active operations, it may be necessary to obtain command of the air. I am a little doubtful whether this is practicable. When one considers the very high speeds and the great manœuvring power of aeroplanes, I am not yet clear that they will be able to fight each other effectively, or that it will be possible to use numerical superiority of aircraft to prevent reconnaissance work on the part of a daring opponent. For a time, at all events, the fast scout will have the advantage, as Colonel Sykes admits; but, until armies well equipped with aeroplanes have met, I do not believe that the point can be settled. Speaking broadly, the naval and military effect of aeroplanes must be immensely to extend the field of vision of a Commander-in-Chief. The aeroplane cannot be an ideal observing station, and the experience of our manœuvres has shown that serious mistakes are possible. Every plane must carry a trained observer, who will have nothing else to do. I think that any important distant reconnaissance should be carried out by at least two planes working together, so as to obtain independent observations. I understand that the difficulty of observing to a flank is considerable, and I imagine that, in case of any doubt, the aviators would circle, so as to secure varying points of view. If wireless apparatus can be carried, valuable time may sometimes be saved. I do not believe that aerial reconnaissance will in the least diminish the importance of cavalry, but it may render long-distance

work unnecessary, thus saving horseflesh, obviating the necessity for large detachments, and opening out opportunities which men on tired horses are not capable of utilising. An aeroplane would have been invaluable to Lord Roberts when his great movement to turn General Cronje's position began. If the direction of the Boers' retreat had been known in time, the cavalry would have been spared an exhausting experience, and some subsequent events might have been very different. For the sustained observation of artillery fire, I do not think the aeroplane will be able to compete with the captive balloon; but it is far more handy and more easily transported, and it has already proved useful for this purpose. It will not be possible to prevent the egress of aeroplanes from a foreign fortress, and it may be difficult to prevent their ingress. Looking back on history, one is forcibly struck with the differences that the employment of aeroplanes might have made at critical moments. Suppose they had been available at Napoleon's Headquarters on the morning of the 17th June, 1815. Wavre, where the Prussians were collecting, was well under twenty miles away. A smart observer might have discovered what they were doing and brought back the news in an hour. The orders tardily given to Grouchy to march on Sembloux, which took him far away from the scene, would never have issued. Could he then have held Blucher fast at Wavre, and if he could, what would have happened at Waterloo? On the 4th June, 1805, Nelson arrived at Barbadoes, and received the false information that the French fleet had gone south, which took him down to Trinidad. If General Brereton, at St. Lucia, had had an aeroplane, the truth would have been known, and Trafalgar might have been fought in West Indian waters. These are only speculations. What is certain is that, just when the wide extension of the area of military operations and of the battle fronts of armies was thought to have limited the powers of Commanders-in-Chief, and necessitated great independence on the part of subordinate Generals, a new means alike of reconnaissance and of communication over great distances has arisen. Power has been given back to the Commander-in-Chief, and with it greater demands will be made upon his judgment. The possibilities open to him have increased, the results of his mistakes, if he makes them, must be more serious. As General Henderson has said, he had better keep his aeroplanes under his hand, at least while the enemy is at a distance. May we not, therefore, assume that aviation, like so many other products of scientific advance, will confer enhanced importance upon the most highly trained intelligences, and still more upon genius. The Nelsons and Napoleons of the future will obtain from it greater advantages than their less gifted opponents.

You will all have noticed the interesting discussion of military aeronautics in the French Senate yesterday. I will not enter upon the question of relative strengths in aircraft. Arithmetical calculations are often misleading. But we may safely lay down as a principle that a nation's strength in the new element of warfare must depend upon the number of craft perfectly fit for immediate employment and upon the number of highly trained pilots and observers. Having regard to the possibility of dangerous mistakes, to which I have referred, I believe that the careful training of military observers is a vital point.

In conclusion, I wish to express my conviction of the national importance of the work which the Aeronautical Society is carrying on. It is necessary in this country, especially at the present time, to keep naval and military needs constantly before the not too enlightened mind of the man in the street. By explaining what is being done, by discussing the conditions and possibilities of aeronautics, by securing co-operation between civil scientists and the Services, and by reminding Governments of their responsibilities, I believe this Society is doing very valuable and patriotic work. We were characteristically slow in making a start. We are now, I hope and believe, making up some of our leeway. Both General Henderson and Mr. Holt Thomas have given us hopeful information. Mr. Holt Thomas has stated that, in regard to personnel, we are superior to our rivals, and I hope

we are now in a fair way to make up for lost time, and to obtain the eminence which Colonel Sykes demands. I am glad to note that a tardy beginning has been made in India, where aeroplanes may be of the greatest use.

The Society could have found many chairmen far more competent to preside at this meeting than I; but none could have been more deeply imbued with a sense of the importance of the subject. I have much pleasure in proposing a very hearty vote of thanks to Colonel Sykes for his most interesting and instructive lecture.

The Lecturer briefly replied.

General RUCK, in proposing a vote of thanks to Lord Sydenham for the honour he had conferred on the Society by taking the chair, referred to his eminence as an authority on Imperial Defence, which made his complimentary remarks concerning the Society's work all the more gratifying and valuable. He could assure him that they appreciated very much his kindness in presiding. The motion was carried with acclamation, and the meeting terminated.

THE SEVENTH MEETING of the FORTY-NINTH SESSION of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 18th February, 1914, at 8.30 p.m. Major-General R. M. Ruck, C.B., R.E. (retd.), presided.

PROPELLERS:

BEING A CO-ORDINATION OF SOME EXPERIMENTAL RESULTS AND THE DEDUCTIONS DRAWN FROM THEM.

BY FRANCIS H. BRAMWELL, B.SC., ASSOCIATE FELLOW.

The material for this paper was collected for a written communication to the Research Committee for publication in THE AËRONAUTICAL JOURNAL, in the form of a brief annotated account of the work of a number of experimenters on the performance of the screw propeller in air and water; but, at the request of the Council, a month ago, the article has been re-cast in the form of a paper.

As can be readily understood, such a catalogue would be extremely boring to those who have so kindly come here to-night to listen, so that it was considered preferable to give only some general conclusions with sufficient experimental evidence to attempt to justify them.

The short résumé has, however, been retained in the form of an Appendix, for the use of those who desire to refer to it.

It was hoped that, by keeping the paper as short as possible in this manner, there would be plenty of time for the meeting to hear the opinions of the many able members who have had experience of this subject. It was also felt that a free discussion would be the best way of clearing up many little differences of opinion, caused very often by the failure of one man to appreciate the point of view of another.

After these few preparatory remarks, it is proposed to deal first with the question of corresponding speeds, which includes the consideration of how far results obtained in water or on model propellers in air or water may be taken as applicable to full-size air propellers.

The Application of the Laws of Similitude.

A number here to-night will probably remember Mr. Bairstow's paper on "The Laws of Similitude," read before this Society last year; the method of dimensional equations that was referred to in that paper has been extended to include the case of propellers in an article in the Report of the Advisory Committee for Aeronautics for 1911-12.

It is necessary, in order to make a strict comparison between the results obtained from any two propellers, that they should be exactly similar; that is, the one propeller should be, as it were, a photographic copy of the other, either larger or smaller.

This being so, the expressions for the thrusts of the two propellers, working in different fluids, are

$$t = \rho_1 l^2 v^2 f\left(\frac{v}{ln}, \frac{vl}{\gamma_1}, \frac{v}{V_1}\right)$$

$$\text{and } T = \rho_2 L^2 V^2 f\left(\frac{V}{LN}, \frac{VL}{\gamma_2}, \frac{V}{V_2}\right)$$

where l and L are any similar lengths in the two propellers, and may be taken as the diameters; v and V are their translational or forward speeds; and n and N their rotational speeds. ρ_1 and ρ_2 are the mass densities, and γ_1 and γ_2 the kinematic co-efficients of viscosity of the two fluids; $\mathbf{V}_1$ and $\mathbf{V}_2$ are the velocities of sound in the two fluids, these being a measure of the compressibility in the two cases.

f is simply a general function of the three independent, nondimensional variables $\frac{v}{ln}$ $\frac{vl}{\gamma}$ and $\frac{v}{\mathbf{V}}$

Similar expressions to the above can be written down for the torques on the two propellers.

These formulæ look sufficiently complicated to make it appear difficult to see how they can be simply applied. Some experimental evidence will be brought forward, however, to show that the dependence on the viscosity and compressibility is so slight as not to introduce any serious errors if it be neglected entirely; the experimental evidence available on this point is still somewhat scanty, but that which has been collected confirms this view.

Under these circumstances the above equations reduce to

$$t = \rho_1 l^2 v^2 f\left(\frac{v}{ln}\right) \text{ and } T = \rho_2 L^2 V^2 f\left(\frac{V}{LN}\right)$$

which may be re-written

$$\frac{t}{\rho_1 l^2 v^2} = f\left(\frac{v}{ln}\right) \text{ and } \frac{T}{\rho_2 L^2 V^2} = f\left(\frac{V}{LN}\right)$$

If now the propellers be compared, when they are revolving in a similar manner, which is expressed by the statement that $\frac{v}{ln}$ is equal to $\frac{V}{LN}$, then it is seen that $\frac{t}{\rho_1 l^2 v^2} = a$ (a non-dimensional number) $= \frac{T}{\rho_2 L^2 V^2}$

And under the same conditions, we have for the torque $\frac{q}{\rho_1 l^3 v^2} = b$ (a non-dimensional number) $= \frac{Q}{\rho_2 L^3 V^2}$

Also the efficiencies of the two propellers are the same, being $\frac{a}{2\pi b}\frac{v}{ln}$ and $\frac{a}{2\pi b}\frac{V}{LN}$ respectively, which are equal as $\frac{v}{ln}$ is equal to $\frac{V}{LN}$

The use of these two non-dimensional numbers or co-efficients is so convenient that it is desired to draw special attention to them. As has been pointed out, they are independent of the size and speed of the propeller, and also apply equally well to water and air, so long as the square law holds sufficiently well. They have also another special advantage; that is, being non-dimensional, they have the same values in any system of units, and are thus available for comparing directly the results of tests carried out in any country. The only essential is that the units employed must be consistent among themselves.

For expressing the results of static tests, or tests at a fixed point, where v is zero, two other co-efficients are suggested; these are

$$a_o = \frac{t}{\rho l^4 n^2} \text{ and } b_o = \frac{q}{\rho l^5 n^2}$$

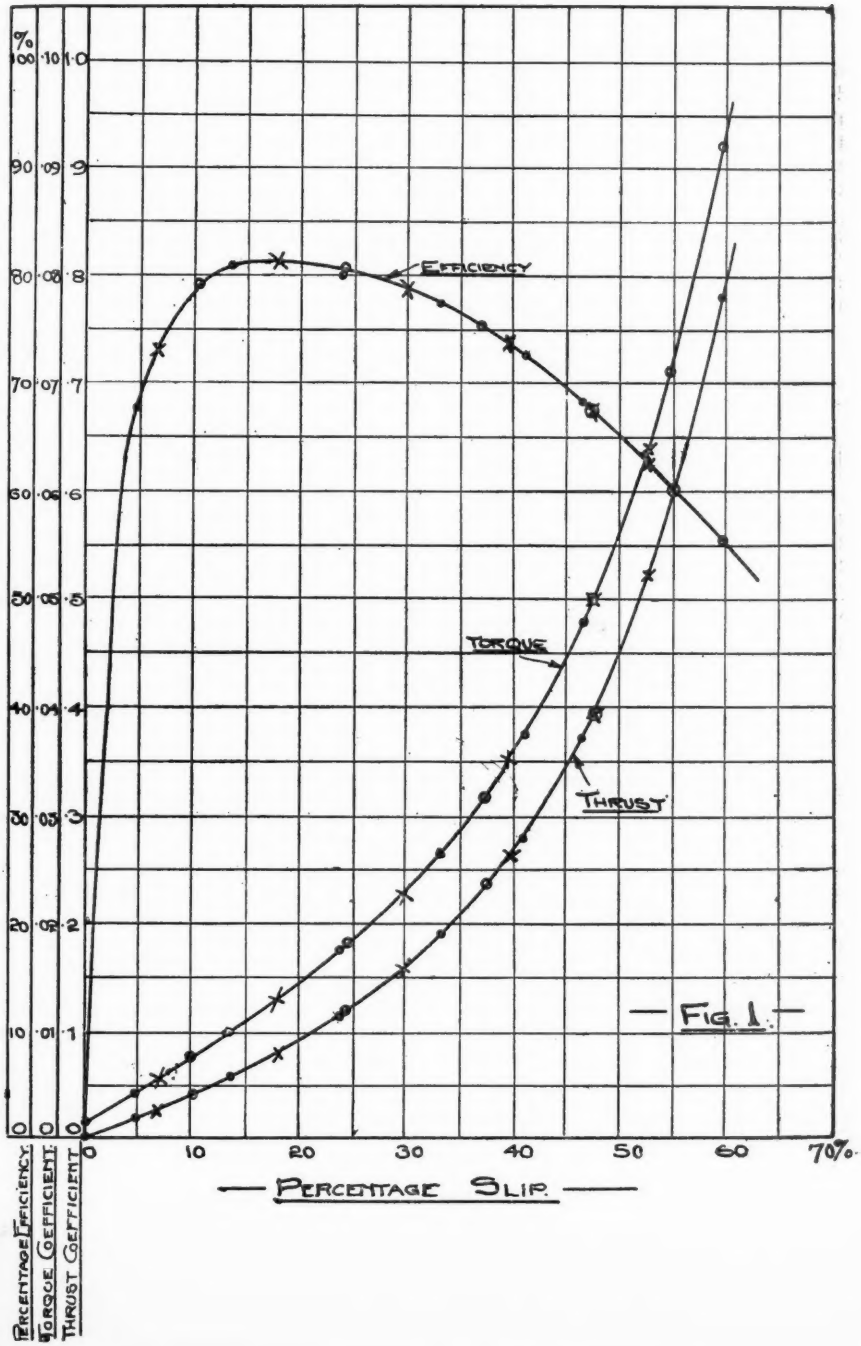
Not only are these co-efficients non-dimensional, but they are constant for similar propellers of any size and at any speed and in any fluid, so long as the square law holds sufficiently closely.

Evidence of Non-Importance to Screw Propellers of Viscosity and Compressibility of Air or Water.

Turning now to a consideration of some of the experimental evidence supporting the view that the effects of the viscosity and compressibility of both water and air may be neglected in so far as they affect the performances of screw propellers, results obtained on similar propellers in water were published by D. W. Taylor in 1906, three sets of five propellers of different diameters being tested. These are discussed somewhat fully in the Appendix, where it is shown that the differences obtained are only slight, and are probably covered by experimental errors. The range of diameter in these experiments was three to one. Several experimenters have published isolated cases of comparisons between two similar propellers working in air. Thus E. Dorand in 1910 gave particulars of tests, with two similar propellers, of 2.5 metres and 4.3 metres in diameter respectively, carried out on a moving carriage, at speeds up to 12.5 metres per second. He showed that the forces on the two propellers varied as the square of the speed and the square of the diameter very closely indeed, for the same value of the ratio $\frac{v}{ln.}$ G. Eiffel in 1911 also showed that this was true to a close approximation, but the precision of his experiments was sufficient to indicate a slight discrepancy in some cases. This discrepancy was chiefly attributable to the distortion of some of the propeller blades at high speeds; and it is interesting that the results from two propellers of 2.97 feet and 8.90 feet diameter agreed almost exactly, when they were tested at the same tip speed, at which the deformations of the two propellers were the same. This can be shown very easily by a further extension of the method of dimensional equations.

In the Report of the Advisory Committee for Aeronautics for 1910-11, one comparison was given between the results of tests with a model propeller, two feet in diameter, on the whirling arm at the National Physical Laboratory, and with a similar propeller, fifteen feet in diameter, on Messrs. Vickers' whirling arm. The results showed very good agreement indeed. The speaker himself has not carried out any tests on similar propellers of different diameters, but has tested the same propellers at different speeds, which is exactly equivalent. The results of such tests on a particular propeller are shown in the following figure.

Here the thrust has been divided by $\rho l^2 v^2$ and the torque by $\rho l^2 v^2$. In the figure, curves of these thrust and torque co-efficients and of the efficiency have been plotted against the percentage slip, which is merely a function of the ratio $\frac{v}{ln.}$ The open circles refer to tests at a translational speed of 1,800 feet per minute, the crosses to 2,200 feet per minute, and the black dots to 2,600 feet per minute. It is seen that the points all lie extremely well on one set of curves, which shows that the departure from the square law is extremely small within the range of speed considered. It is admitted that this speed range is only small; but the experiments on this point are so few that every bit of evidence must be considered; and from a consideration of all the evidence available it is felt to be justifiable to say that there is none at present to indicate that any serious errors will be introduced if propellers are assumed to follow the square law exactly. This is especially so, as the difficulties in most propeller tests are so great as to make it very hard to discover experimental errors; and where slight departures from the square law have apparently been discovered, it is generally found that the experimental errors are very likely of the same magnitude at least. Before leaving the consideration of this figure, attention may be drawn to one or two points.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PERCENTAGE SLIP
PERCENTAGE EFFICIENCY
TORQUE COEFFICIENT
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— FIG. 1 —

Relation between Efficiency and "Slip."

It is generally found that the higher the maximum efficiency of a propeller the less the value of the slip at which it occurs. This means that with a propeller giving a high maximum efficiency, the thrust corresponding to the conditions of maximum efficiency is so small that advantage cannot be taken of it in an actual aeroplane. From a consideration of the conditions under which the propellers on actual aeroplanes work, it has been found that the usual value of the thrust co-efficient lies between 0.15 and 0.20; hence the efficiencies of different propellers should be compared when they are operating between these limits of thrust. The results given in the figure show that this particular propeller is very good in this respect, as the efficiency at a thrust co-efficient of 0.18 is still 78%. Another point, which might with advantage be impressed on many would-be inventors, is that for this propeller the maximum efficiency is about 82%; and there are probably very few aeroplane propellers in use at the present day that do not reach a maximum efficiency of 70%, and an efficiency under working conditions of about 60%. This tends to make one very suspicious of inventions claiming to improve the efficiency of propellers by any considerable percentage.

What is the "Pitch" of a Propeller?

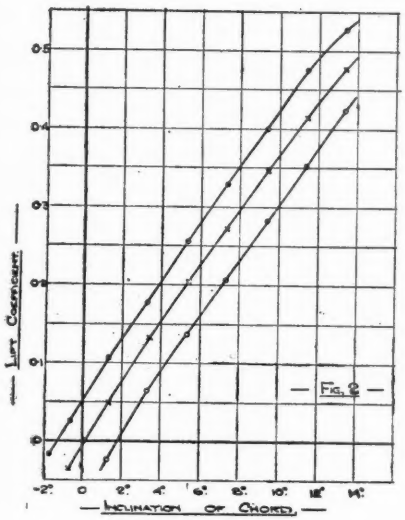
It is now proposed to discuss briefly the question as to what should be taken as the pitch of a propeller; most people have their own views as to the meaning of this term, and this is one of the points on which the speaker expects to have to defend his conclusions against a number of criticisms. Therefore the arguments to show that the value adopted is the most rational one have been given somewhat fully.

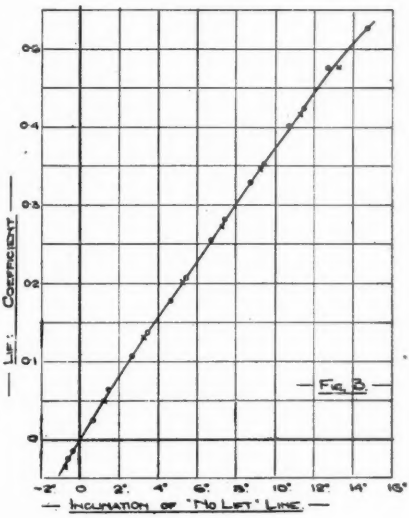
If two propellers of the same diameter be tested, no details of the design being known in either case, and it is found that the thrust and efficiency are the same for each, at the same values of translational and rotational speeds; then a rational value of the pitch is defined as that value, which makes the curves of performance of the two propellers identical.

The definition of pitch adopted is that known as either the experimental mean pitch, the conventional pitch or the aerodynamic pitch, which is the advance per revolution of the propeller when it is giving no thrust. Another definition of pitch very largely used is obtained by considering the pressure face of the propeller only. Then if a narrow strip of the blade be considered, at a distance r from the axis of the propeller and with the chord of the pressure face inclined at an angle ϕ to the plane of the propeller, the pitch of this strip is defined as $2\pi r \tan \phi$, which is the advance of the strip per revolution, when the pressure face only is considered as screwing itself into a solid medium. In most propellers used in aerial work this pitch is different at every radius, and thus a mean value has to be taken to express the pitch of the complete propeller. It is not certain how this mean should be taken, as the efficiency of the blade, from the point of view of thrust producing power, increases with the radius.

Now consider two propellers of the same diameter, with the pitch, as defined in this manner, the same in the two cases. But let one of them have thin blades and the other much thicker blades; then the performances of the two propellers, at the same value of the slip, will be very different in the two cases; and the value of the advance per revolution, to give no thrust, will also be very different.

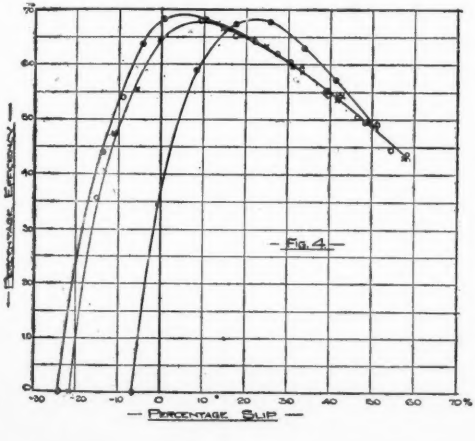
It is very analogous to the case of two aerofoils for which the rate of increase of lift with angle is the same, but one starts to lift at a less inclination of the chord than the other. This is shown for three aerofoils in Fig. 2, where the lift co-efficients, at the same wind speed, are plotted against the inclination of the chord to the wind direction.

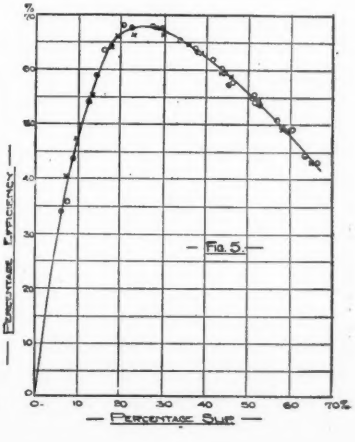




If the lifts of these three aerofoils be compared at the same inclination of the chord to the wind direction, their performances are apparently not at all equal. But an inspection of the figure indicates that the performances are very similar, and this is brought out clearly if the curves are plotted against inclinations measured from the position of no lift. This has been done in Fig. 3, from which it is apparent that the behaviour of the three aerofoils is practically the same in the three cases.

Similar reasoning applies to the case of propellers. In Fig. 4 are shown the efficiency curves obtained for three different propellers plotted against the slip determined by using the mean chord pitch. Here again, from a comparison of the efficiencies of the three propellers at the same value of the slip, there is apparently a large difference between them.





The curves have, however, been replotted in Fig. 5, using the experimental mean pitch to determine the slip; and from these it is seen that the efficiency curves are extremely alike, and there is very little to choose between the three propellers.

In Fig. 4 it is seen that the curves for two of the propellers are fairly close; this is merely because the blade sections are almost identical for these two propellers.

The ease of comparison between different propellers, obtained by using this experimental mean pitch, chiefly accrues from the fact that the curves for all propellers are made to agree at one value of the slip, that is for zero value of the slip; at which value both the thrust and efficiency of any propeller are zero, which is itself an additional advantage.

Having now pointed out some of the advantages of the use of this value of the pitch, it remains to examine the objections that may be taken to it.

It is claimed that, when once the method of calculating the mean of the chord pitches of a propeller at the different radii has been determined, this mean chord pitch has one definite value for a given propeller; and it has been urged that the experimental mean pitch varies for a given propeller at different speeds.

If this contention be examined theoretically, however, the equation previously given for the thrust $\left[t = \rho\, l^2\, v^2\, f\left(\frac{v}{ln}\right) \right]$ shows that the value of the ratio $\frac{v}{ln}$, which makes the thrust zero at any speed, does so at all speeds.

And, therefore, if the square law assumptions hold with sufficient accuracy, the experimental mean pitch is constant; that is, it is a constant multiple of the diameter, in the same way as the mean chord pitch.

Another way of putting the same thing is that the closeness of the experimental mean pitch to a constant value at different speeds is only a further indication of the degree of approximation to which the square law assumptions are true.

In the course of his paper before this Society last year, Mr. Low gave some figures taken from the Bulletin of the Koutchino Laboratory, which only showed a variation of $\pm$ 2.5% in the value of the experimental mean pitch over a considerable speed range; this variation is probably covered to some extent by experimental errors.

In the course of the propeller experiments carried out at the National Physical Laboratory, it has been found invariably that the experimental mean pitch for a propeller is constant within the limits of experimental accuracy for a speed range of 15 to 50 feet per second. Very little experimental data is available on this particular point, but that which has been quoted indicates that the departure from the square law is very slight under these conditions, and that the experimental mean pitch may be taken as constant for practical purposes.

Another objection advanced is that it is impossible to determine the value of the experimental mean pitch during the course of the design of a propeller. But recent experiments all tend to show that the value obtained by actual test differs very slightly from that obtained by calculation from the no lift lines of the various sections, which are used instead of the pressure face chords to determine a mean value of the pitch for design purposes.

Methods of Propeller Design.

It is well to give a little consideration to the various methods that are in use for designing propellers; these may be divided roughly into two groups. The first of these, which has been used for the design of almost all existing water

propellers, is to design from previous experience. When a new propeller is required to work under conditions differing slightly from any previously encountered, a propeller that has actually proved itself to be successful under somewhat similar working conditions is chosen as a model. The new propeller is then designed by making slight alterations from this selected model.

This method has proved very successful for marine propeller work, the only limitation in the method being that the variation made between any two successive propellers must, of necessity, be small.

It must not be forgotten that there is, at the present time, a big difference between the sciences of marine and aerial propulsion, which difference is merely a question of the relative ages of the two sciences.

No doubt one of the chief reasons for the success of this method, when applied to the design of water propellers, is that the number of different propellers working successfully under various conditions is very large; and it is generally an easy matter to select a propeller that shall serve as a model for slight alterations or variations. But aerial propulsion is not yet in this happy state; the science is so new that there has not been time for sufficient data to accumulate to allow this method of design to be applied with certainty of success.

The Aerofoil Analogy.

The result has been that a new and more powerful method of design has been evolved by aeronautical engineers; this method may be termed the blade element theory. This was first developed by Lanchester and Drzewiecki, but has since been considerably modified by subsequent designers. The method is so well known and was so ably expounded by Mr. Low, in a paper read before this Society last year, that it is only necessary to mention the chief points at present.

The root assumption underlying the theory is that any small element of a propeller blade (cut off by two cylinders, concentric with the shaft) is subject to the same forces as an element of an aerofoil of equal length (cut off by two planes parallel to the wind direction), the resultant velocity of the element and the inclination of the element to the direction of the resultant velocity being the same in the two cases. In the case of the propeller blade element the velocity considered is the resultant of the translational velocity and the peripheral velocity at the radius of the element. It is immediately apparent that this is a very far-reaching assumption, and it is now necessary to investigate to what extent it is justified by the results obtained. This practical test, as to whether the method gives satisfactory results when carefully applied, must always finally decide the application of theories of this kind.

The method, as developed by Drzewiecki and others, goes a little farther than the above assumption. It is shown that the efficiency of a propeller will be a maximum, when every element is inclined at such an angle to the direction of its resultant motion that, if used as an aerofoil, it would give a maximum value for the ratio of lift to drag.

Hence it has been usual to assume a sort of mean section for the propeller blade; and, from experiments on aerofoils in a wind channel, to determine the inclination of this mean section, such that it will give a maximum value of the ratio of lift to drag.

The propeller is then so designed that, under its working conditions, every element of the blade is inclined at this best angle of attack to its resultant velocity, so that under these conditions the over-all efficiency of the propeller shall be a maximum.

The forces on each element can then be determined and the total force on the propeller blade be obtained by integration. In Mr. Lanchester's method the

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integration is graphical in order that the width of the blade may be varied to fit in with other considerations.

Drzewiecki however worked out his equations on the assumption of a blade of constant width; and Mr. Low last year showed us the equations with the width of the blade expressed as a function of the radius. The object in both these cases was evidently to obtain expressions for the forces on the elements of the blade that could be integrated without resource to graphical methods.

The speaker would like to put in a plea for a further extension of the method, as one strong objection to the method in its present form is that the section of a large portion of the blade must differ very considerably from the mean section taken; this is necessary from considerations of strength and stiffness.

But results of tests on so many aerofoils, of very widely varying sections, have now been published, that it should not be difficult to pick out a series to represent almost any propeller blade at various points along the radius. The resultant force on the blade could then be obtained by graphical integration with a much closer approximation to its actual value than is possible by the method of taking a mean section for the blade.

If this source of error be eliminated in this manner, the comparison between the forces obtained on a propeller and those obtained by integration from the results of tests on the appropriate sections in a wind channel would be a determination of the extent to which the root assumption mentioned is justified by experiment.

The Aerofoil Analogy Tested by Experiment.

In order to test this the speaker carried out a series of experiments last year at the National Physical Laboratory; the detailed account of these experiments will be published very shortly now in the Report of the Advisory Committee for Aeronautics. Without going into much detail, it is proposed to give a short account of these experiments and the deductions that may be drawn from them.

The propeller used was two-bladed and two feet in diameter; only one set of blades were used, but these were mounted so that they could be rotated, and the tests were carried out with the blades in four positions. The propeller was very similar in design to the Normale propellers, such as are designed for Messrs. Ratmanoff Fréres by Drzewiecki. For the purpose of most of the work, the use of four positions of the blades was equivalent to the use of four distinct propellers, and yet errors in workmanship between the different propellers were eliminated by this means. The mean section of the blades was such that the maximum value of the ratio of lift to drag was obtained with the chord inclined at approximately 4° to the resultant velocity. The propeller was so designed that, at one particular value of the ratio of speed of translation to speed of rotation, the chords of all parts of the blades were moving parallel to their resultant velocities.

Tests were carried out with the blades in this position, and also with the blades rotated successively through 4°, 8° and 12°. Then for the one particular value of the ratio of the speed of translation to speed of rotation, all parts of the blades were moving with their chords inclined at 4°, 8° and 12° respectively to their resultant velocities. Then, according to Drzewiecki's equations, the propeller should give its maximum efficiency with the chord of every portion of the blade moving at 4° to its resultant velocity.

From the tests on the four propellers, it was found that one with an extra angle of approximately 9° gave the maximum efficiency; but a consideration of the results showed that, when this propeller was giving its maximum efficiency, the angle of attack of the whole of the outer half of the blade varied by less than one degree from the theoretical value of 4°; and the further work carried out

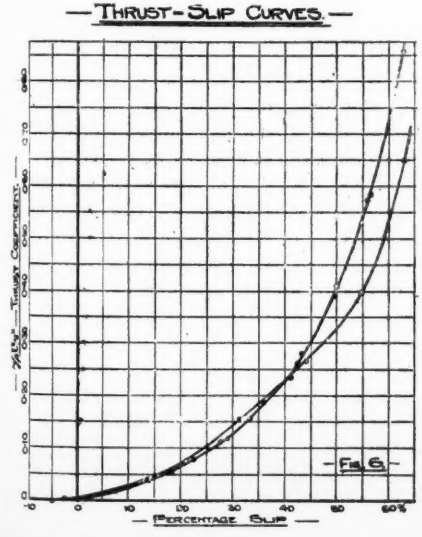
showed that the effect of the blade inside half radius was only a small fraction of that of the outer half. Also the whole difference in the maximum efficiencies of the propellers with the extra angles of 4° and 9° was only 2.5%. Therefore in this respect, the tests indicated that the agreement was quite reasonably close.

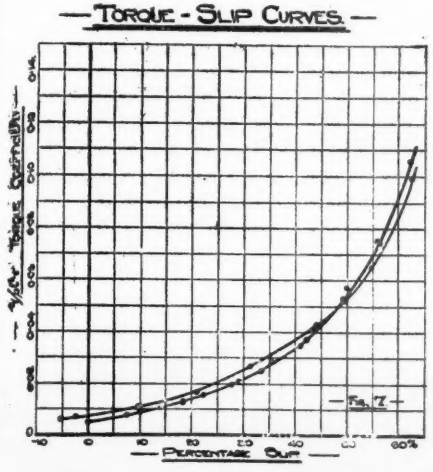
In order to complete the more exact comparison on the lines already suggested, a series of aerofoils were made to exactly the same sections as those employed for the propeller blades. Six sections in all were made and tested, these being taken at every two inches of radius of the propeller. In order to eliminate errors of workmanship as much as possible, the same templates were used in the manufacture of the propeller blades and of the aerofoils. Also, to avoid the effects of distortion on the propeller blades, no attempt was made to design the most efficient propeller; but the blades were made sufficiently stiff not to suffer any appreciable distortion during the tests, although a more efficient propeller could have been designed by adopting thinner blades.

As is well known, the aspect ratio of an aerofoil has a very considerable effect on the values of the forces obtained on it at any particular speed, so that it was necessary to make some assumption with regard to the aspect ratio to be adopted for the aerofoils. This was done somewhat arbitrarily by making each aerofoil of the same width as the propeller blade at the appropriate section, and one foot long, that being the length of one propeller blade.

These six aerofoils were then tested in the wind channel, and measurements made of the lift and drag forces for different angles of attack, in a wind of 30 feet per second. From these figures the forces on the blades were obtained by graphical integration for the four propellers at various values of the slip. Allowance was of course made for the different speeds of the various sections. In making the calculations, reliance was not placed on the designed inclinations and angles of attack of the various sections, but the actual inclinations of the blades at the various radii were accurately measured up, both before and after the tests, and the mean values so obtained were used in the calculations. This eliminated errors due to the warping of the blades, either from the forces on them during the tests or from damp and temperature changes in the atmosphere.

It is not proposed to give here the detailed results of these experiments, but Figs. 6, 7 and 8 have been prepared to show the type of agreement obtained on one particular propeller.

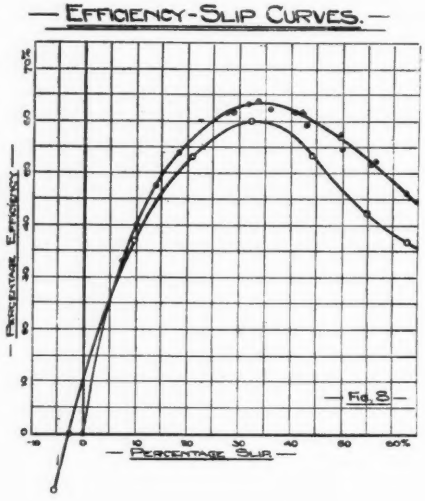




These show the values of the thrust and torque co-efficients and the efficiency plotted against the slip. The black dots are the experimental values and the open circles the values obtained by integration.

In order to make the comparison correct, the same value of the pitch has been used in determining the slip for both sets of results; for this purpose it is immaterial what particular value is used, so long as it is the same in the two cases, for the conditions are fixed by the ratio of the speed of translation to the speed of rotation and the pitch is merely a convenient factor in the reduction and presentation of the results.

From the curves it is apparent that the agreement is not exact; but as it has been generally stated by several experimenters that Drzewiecki's theory is in good agreement with experimental results, it appears necessary to examine whether such discrepancies as are here apparent would be detected by any method of comparison previously applied.



In the first place, it is not necessary to make the comparison over the whole range of slip, as, from consideration of the conditions under which the propellers on actual aeroplanes work, it has been found that the usual value of the thrust co-efficient lies between 0.15 and 0.20; this limits the range for comparison very considerably.

It is seen that the efficiency discrepancy is chiefly due to the calculated torque being greater than that obtained by experiment. It is believed to be usual for propeller designers to allow in their calculations for these discrepancies by making fixed percentage corrections to the values of the lift and drag co-efficients obtained from experiments on aerofoils.

There is one other point that is of interest in this respect which can be best illustrated by means of a concrete example. Suppose the propeller, the results for which are shown in the figures, to have been designed to give a thrust co-efficient of 0.164 at a slip of 32.5%, and let the engine speed corresponding to this slip be 1,150 revs. per minute.

An inspection of Fig. 6 will show that under these conditions the actual thrust co-efficient obtained would only be 0.150. If, however, the engine speed

be allowed to run up until the slip is 34.0%, the required thrust co-efficient of 0.164 will be obtained. The new engine speed under these circumstances will be

$$1{,}150 \left(\frac{1 - 0.325}{1 - 0.340}\right) = 1{,}176 \text{ revs. per minute.}$$

In other words the increase of thrust of 9.3% has been obtained by an increase in the engine speed of 2.3%. It would be easy to suppose that there was very close agreement between the calculated and experimental values under such circumstances, or if the matter be regarded from another point of view the discrepancies between the calculated and observed values are not of great importance, as a small variation in engine speed will bring the thrust co-efficient to the required value. If the engine had been designed from the calculated values, it would have to be run with the throttle slightly closed, as the experimental efficiency would be higher than that calculated.

Causes of Discrepancies in Test Results.

It is well to note a few of the most obvious causes of the discrepancies that are observed in these tests. In addition to the question of aspect ratio, which has already been mentioned, the air speed in the channel experiments was only 30 feet per second, or 1,800 feet per minute; whereas the resultant velocities of some of the sections were very high (the tests being carried out at translational speed of 1,800, 2,200 and 2,600 feet per minute, with, of course, much higher values for the peripheral speeds of the blade tips); this may account for some of the reduced efficiencies.

On the other hand, it is more probable that most of the discrepancies are due to the centrifugal forces on the air in contact with the blade; this must alter the character of the flow round the blade very considerably, and it is perhaps a matter for surprise that the agreement between the calculated and experimental values is as close as it is, and not that they do not agree exactly. In the complete account of the tests some further minor comparisons were made; but perhaps enough has now been said to show that, although the method is not strictly correct, yet in the hands of a careful designer it is probably by far the best method that can be used for the design of aerial propellers, in the present state of knowledge on the subject.

APPENDIX.

A COLLECTION OF EXISTING EXPERIMENTAL RESULTS ON THE PERFORMANCE OF THE SCREW PROPELLER IN AIR AND WATER.

It is hoped that the following collection includes most of the results obtained by the various experimenters in this branch of the problem of marine and aerial propulsion, and that it may be of some use to others intending to take up the study of this interesting subject.

In order to make the paper a little more complete, a short list of books and papers, dealing with the study of propellers, has been appended.

The work on water and air propellers has been separated as much as possible; that on water propellers, as being the older, being taken first.

So many different symbols are in use by the various writers on propellers

that some confusion is liable to creep in. The writer suggests the following list as retaining the best and most commonly used of these many symbols:—

T or t	thrust (lbs.).
Q or q	torque (foot lbs.).
N or n	rotational speed (revs. per min.).
V or v	translational speed (feet per min.).
H.P. or h.p.	horse-power absorbed by the propeller.
P or p	pitch ratio of propeller $\left(\frac{\text{pitch}}{\text{diameter}}\right)$
D or d	diameter of propeller (feet).
L or l	
S or s	slip ratio $\left(\frac{pdn - v}{pdn}\right)$
E or e	efficiency ratio.

The use of the capital and small letters is extremely useful in comparing results on model propellers with those on full size propellers, as one set of letters can be kept for each propeller.

The writer has also found the use of non-dimensional co-efficients very useful for expressing the performance of a propeller, as, within the limits of application of the "square law," they are independent of the size or speed of the propeller.

The slip ratio and efficiency ratio are such non-dimensional co-efficients, and the further ones suggested are:—

$$a = \frac{t}{\rho\, l^2\, v^2} \text{ and } b = \frac{q}{\rho\, l^3\, v^2}$$

In these two co-efficients ρ is the mass density of the fluid in which the propeller is working.

For expressing the results of static tests, where v is zero, two other co-efficients can be used, namely:—

$$a_o = \frac{t}{\rho\, l^4\, n^2} \text{ and } b_o = \frac{q}{\rho\, l^5\, n^2}$$

These two co-efficients are constant for any size propeller at any speed, so long as the square law holds sufficiently closely.

One great advantage of these co-efficients is that the values are true for any system of units, only provided that the units employed are consistent among themselves.

One other term is used by some experimenters, and this is blade width ratio. This is the ratio of the maximum width of the blade to the radius of the propeller, and is analogous to the reciprocal of aspect ratio, as applied to aerofoils.

Experiments on Propellers in Water.

Most of the systematic study falls naturally under two headings, although several other effects have been studied by different experimenters. The two headings are:— (1) The effect of alteration of pitch ratio; (2) The effect of alteration of blade width.

Froude's Experiments.

The propeller experiments in water that are best known in this country are those of R. E. Froude, carried out at the Haslar Experimental Tank. The results published in 1908[3] supersede the earlier experiments, so that the later results only are considered.

[3] "Results of further model screw propeller experiments" (Proceedings of the Institution of Naval Architects, April 10, 1908).

All the experiments were carried out with the screws in undisturbed water and not behind models of ships, and were all made at one speed of advance only (v = 300 feet per min.).

The diameter of the propeller in each case was 0.8 feet, the boss being made as small as possible, as the effect of a large boss was found to be quite material but somewhat uncertain. The depth of immersion of the screws was kept constant, being 0.64 feet to the centre of the propeller shaft.

Froude has found that the thrust (at a constant translational speed) of any of the propellers which he tested is given by the equation $t = an^2 - bn$ to a very close degree of approximation; so that this equation was used in fairing all the experimental values.

The results of some of the most recent air propeller experiments at the National Physical Laboratory have been compared with this equation and found to agree fairly well, but not closely enough, over a wide range of slip, to justify its use in co-ordinating the results from propellers.

Froude also came to the conclusion that "a" was a co-efficient depending on the dimensions of the screw—*i.e.*, on the blade area and shape of the blade; and also on the pitch ratio.

"b" was a co-efficient depending on the speed and the pitch; this was eliminated in terms of the pitch ratio p and the slip ratio s, the final expression being—

$$t = \frac{a}{p^2} d^2 v^2 \frac{s}{(1-s)^2}$$

so that the thrust results from all Froude's experiments are determined by the changes in the value of "a."

It was found that for changes of pitch (the shape and area of blade being kept constant) "a" was practically proportional to $p(p+21)$.

It was also found that the variation in "a," due to changes of pitch, was practically independent of that due to variation in the shape and size of the blade. So that the value $\frac{a}{p(p+21)}$ (*i.e.*, a constant for varying pitch ratio), was taken as the expression for the blade-factor "B," which was used to denote the thrust capacity of the propeller as dependent on type (such as 3-blade elliptical, etc.), and within each type as dependent on the width proportion of the blade.

The changes in the value of B were given in the form of three sets of curves for the three types of propeller considered, and are here indicated by the following table :—

Disc area ratio.	0.30	0.55	0.80
3 blades, elliptical	0.0978	0.1100	0.1157
3 blades, wide tipped ...	0.1045	0.1182	0.1242
4 blades, elliptical	0.1040	0.1249	0.1318

On comparing the thrusts calculated from the formula with the individual results, it was found necessary to multiply the expression by a correction factor $1.02(1-0.08S)$; so that the final expression for the thrust was

$$t = d^2 v^2 B \frac{p+21}{p} \frac{1.02s}{(1-s)^2} (1-0.08s)$$

Turning now to the efficiencies obtained by Froude; efficiency-slip curves were plotted at convenient pitch ratios for a three-bladed elliptical type propeller of disc area ratio 0.45. For a three-bladed wide-tipped propeller a uniform

deduction of 2% must be made; and for a four-bladed elliptical propeller a uniform deduction of 1.25%.

Deductions must also be made for changes of disc area ratio and pitch ratio.

It was found that:—

For small pitch ratios (say, 0.8), the maximum efficiency fell about 11% for a change of disc area ratio from 0.3 to 0.8. For large pitch ratios (say, 1.4), the maximum efficiency fell about 2% for a change of disc area ratio from 0.3 to 0.8. In both cases the curves connecting maximum efficiency and disc area ratio were practically straight lines. For small slips (say, 15%), the efficiency increased from 62% to 72% for a change in pitch ratio from 0.8 to 1.4. For large slips (say, 40%), the efficiency increased from 61% to 63% for a change in pitch ratio from 0.8 to 1.4.

Froude also found that the effect of a "skewback" or backward rake of 15° was quite negligible both on the thrust and on the efficiency.

The value of the pitch used throughout Froude's work is the advance of the propeller per revolution, when the thrust is zero. In the writer's opinion, this is the most rational definition of pitch, and it is to be hoped that it will be increasingly used, as it enables results from propellers of different pitches to be much more readily compared than by using the pitch as measured upon the pressure face of the propeller.

For propellers of the type used in water this conventional or experimental mean pitch is about 10% to 15% higher than the pressure face pitch. For propellers of the type used in air, it will be in general at least 20% higher.

Taylor's Experiments.

It is now proposed to deal with the work of those two able American experimenters, Taylor and Durand. Taking Taylor's work first, as it is the more widely known, this is contained in two papers read before the Society of Naval Architects and Marine Engineers in 1904 and 1906[1]. All Taylor's work was carried out at the United States Model Basin; and the tests on the effects of blade width and pitch ratio are more complete than those of Froude, as they were carried out at several translational speeds (4, 5, 6, and 7 knots).

It was found that, apart from effects due to cavitation, the results at the various translational speeds were strictly comparable on the assumption that the forces under similar conditions varied as the square of the speed; so that in further experiments on other points only one translational speed of 5 knots was used.

Owing to the troubles with cavitation, the small pitch propellers were only tested up to a speed of 6 knots. It was found that the cavitation was chiefly dependent on the shape of the blade section and the rotational speed, and not on the thrust; so that, for the propellers on which cavitation occurred at all, it would probably occur when the thrust was zero. It is not necessary to consider the effects of cavitation in the case of aerial propellers, as they are working at the bottom of a sea of air that is infinitely deep for all practical purposes.

For the experiments on the effects due to variations in blade width and pitch, a series of thirty propellers was tested. These were all three-bladed propellers, 16 inches in diameter, with a boss diameter of $3\frac{1}{8}$ inches. The thickness of the blades at the root was 9/32 inch and at the tip 3/32 inch.

The pitch on the pressure face was made constant for each propeller, and it was this measure of the pitch that was used for calculating the slip ratio in

[1] "Some recent experiments at the U.S. Model basin" (Society of Naval Architects and Marine Engineers, November 17 and 18, 1904). "Model Basin Gleanings" (Society of Naval Architects and Marine Engineers, November 22 and 23, 1906).

every case. In the opinion of the writer, this method of measuring the pitch lays the legitimacy of many of the conclusions drawn open to question. The writer has, however, attempted to indicate the manner in which the conclusions would have been modified, if the conventional pitch used by Froude had been adopted.

For instance, at no slip the efficiency is very nearly a maximum, especially for the narrower blades, instead of being zero, as it is in all the results published by Froude. Although Taylor thus neglects the back of the blade in taking only the pressure face pitch, he mentions the fact that the back of the blade is very important.

Propellers of six-pitch ratios were tested, namely, 0.4, 0.6, 0.8, 1.0, 1.2, and 1.5. Five widths were taken at each pitch ratio; the disc area ratios were 0.1207, 0.2012, 0.3220, 0.4427, and 0.5635 respectively.

The results are given in the form of curves of efficiency and power co-efficient, plotted against slip. The power co-efficient is practically $\frac{\text{h.p.}}{d^2v^3}$

The tests were taken up to thrusts of between 100 lbs. and 150 lbs. and to slips of about 40%. A maximum efficiency of 78.8% was obtained, which fell off steadily as the pitch ratio was reduced.

As the pitch ratio was decreased the influence of the area of the blades became less, until, for very fine pitch propellers ($p=0.4$), the narrower blades absorbed more power than the wider ones. With $p=0.6$, the power was less with the wider blades below 26% slip, and greater above 26% slip. Even with $p=1.5$ the power did not increase very rapidly with increase in blade area.

In his earlier paper, Taylor described a few experiments on the effect of shape of blade section, the three following sections being tried: —

The diameter in each case was 16 inches, the pitch ratio 1.2, and the disc area ratio 0.3220.

No. 23A showed a slight gain in efficiency, but the power absorbed was much less than by No. 23, being only 50% of that taken by No. 23 under the conditions of maximum efficiency.

No. 23B showed a falling off in efficiency, except at low slips; but absorbed a little more power than No. 23. No. 23 was also run reversed, but the maximum efficiency was less than 50%.

If the conventional pitch had been used, it is probable that the powers absorbed at a given slip would have been very nearly equal, and the efficiencies would have only differed appreciably at their maximum values. Even then, leaving out No. 23 reversed, the variation in maximum efficiency of the three propellers is covered by 5%.

In his later paper, Taylor tested a more comprehensive series of propellers for this purpose. The blade thickness was 1.07 inches at the boss, the diameter being 16 inches, and the pitch ratio 0.7875. The seven forms of blade section tested are shown below:—

In all cases the efficiencies were low, due to the thick blades and the low pitch ratio. Taylor's conclusions generally were that:—No. 140 was better than No. 139, No. 145 better than No. 143, and No. 142 better than No. 144. The best was No. 140, and the worst Nos. 139 and 144.

The whole series of tests is somewhat inconclusive, as Taylor himself suggests that the curves from all these propellers could be made to agree fairly well by using the conventional pitch. The whole variation in maximum efficiency is then covered by about 5%.

Taylor investigated the effect of "rake" on a propeller considerably more thoroughly than Froude. He used a series of six propellers, all three-bladed and

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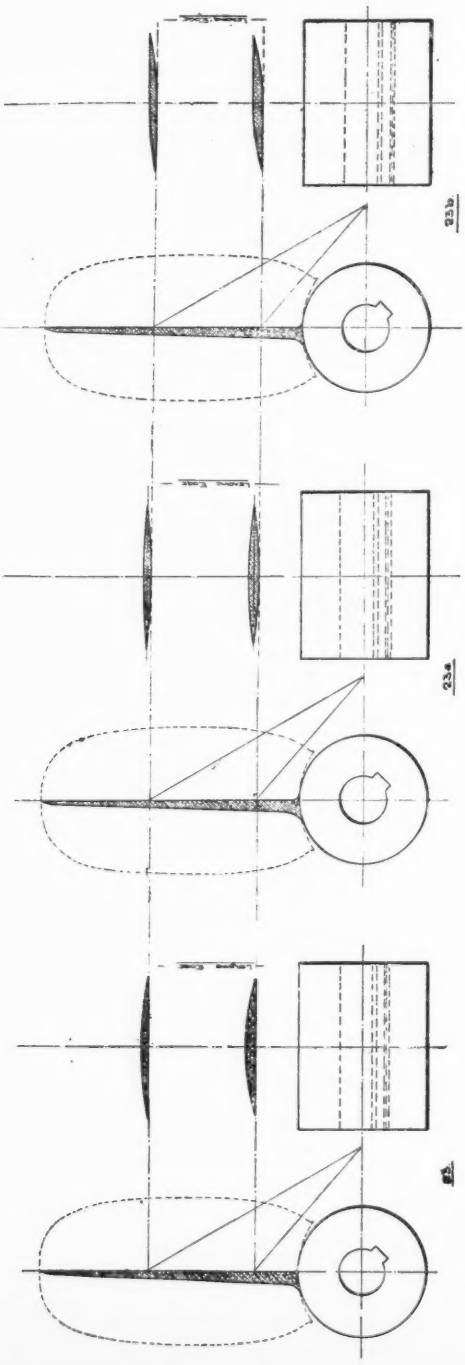

Fig. 9.

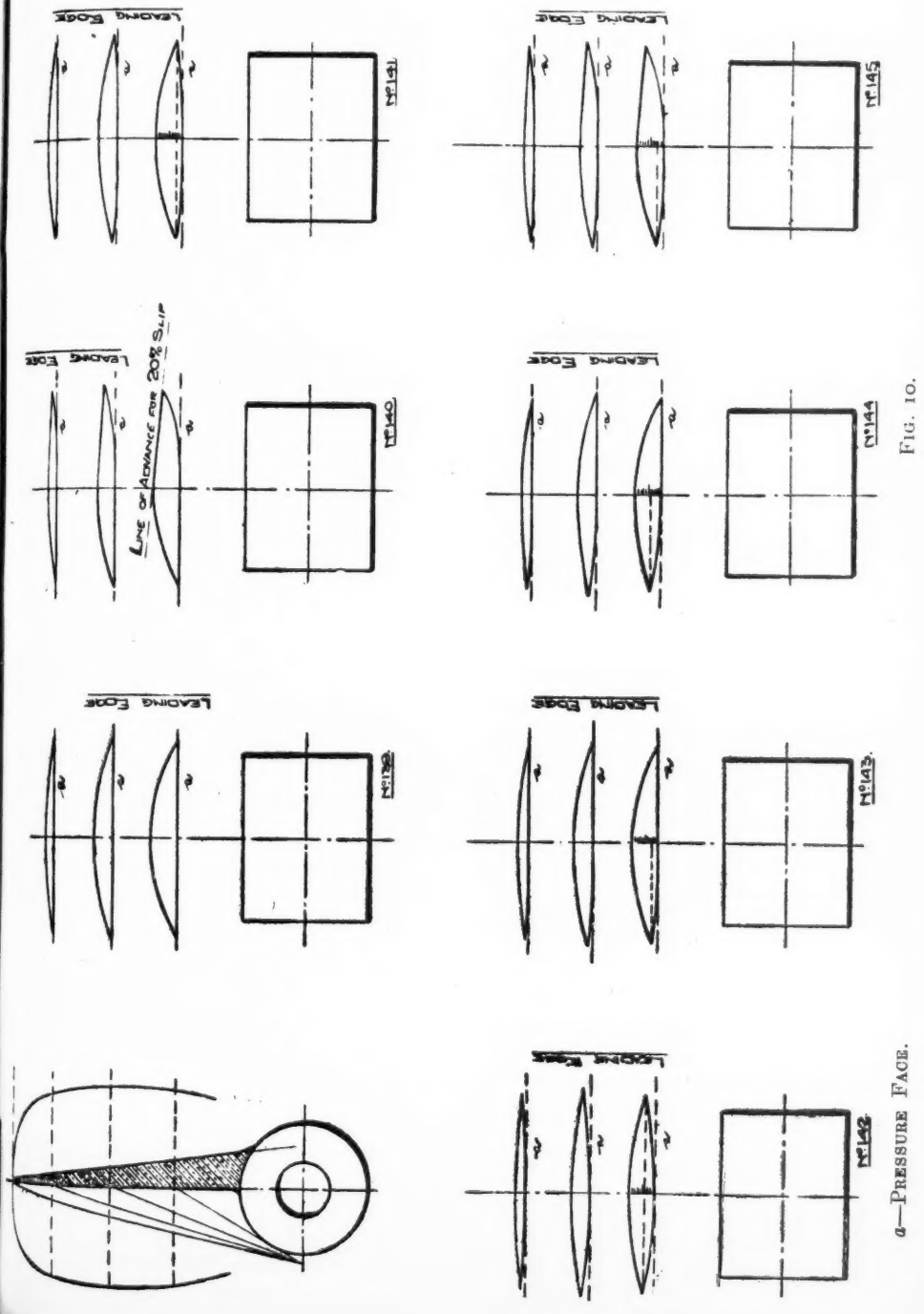

FIG. 10.

a—PRESSURE FACE.

16 inches in diameter, with a disc area ratio of 0.313. These were divided into two sets, one with a pitch ratio of 0.6, and the other of 1.2. Each set then consisted of three propellers, one with no rake, one with a rake of 10° forward, and the third with a rake of 10° aft.

It was found that there was scarcely any effect; if anything, the effect of the forward rake was detrimental.

The effect of variation of blade thickness was also investigated on a series of twelve propellers, all of 16 inches diameter, divided into four sets.

3 propellers; pitch ratio 0.6; disc area ratio 0.196.
3 propellers; pitch ratio 0.6; disc area ratio 0.313.
3 propellers; pitch ratio 1.2; disc area ratio 0.196.
3 propellers; pitch ratio 1.2; disc area ratio 0.313.

The thickness of the blades, considering them as produced to the centre of the shaft, were respectively 9/32 inch, 9/16 inch, and 1 inch in each set.

It was found that there was a decrease in efficiency with increase in blade thickness, due to an increase in the power absorbed, and a less increase in the thrust. Some of this effect is probably due to the pressure face pitch being used in determining the slip.

A further point investigated was the effect of the blade shape. Fifteen propellers were tested, with a diameter of 16 inches, and a blade thickness of 0.92 inches. Five blade shapes were used, three pitches being tried for each blade shape. The values of the pitch ratio were 0.8, 1.0, and 1.2; and the values of the disc area ratio 0.279, 0.289, 0.302, 0.313, and 0.318. In each case the portion of the blade inside the half radius was the same, being an ellipse touching the centre of the shaft. The shape of the outer portion of the blade was determined by the equation $\left(\frac{x}{r/2}\right)^n + \left(\frac{y}{b}\right)^u = 1$; where r is the radius of the propeller, x the distance from the half radius; y the breadth of the blade, and b the breadth at half radius. Values of n were taken as 1.5, 2.0, 2.5, 3.0, and 3.5. $n = 2$ gives an ellipse, and $n = 3$ gives the blade shape previously recommended by Taylor. The broad-tipped blades in general absorbed more power, and delivered more thrust, except at small slips; the curves were very close at small slips, again, probably, due to the method of measuring the pitch.

The narrow-tipped blades were the more efficient, except for very fine pitches. Taylor gives as a general rule (governed by cavitation, which does not affect air propellers), that an elliptical blade should be used for coarse pitches, and a broader tipped blade for fine pitches. The differences in maximum efficiency were never more than 5%, and some of this was probably due to accidental errors.

One very important piece of work undertaken by Taylor was the determination of how closely the square law assumption was justified by experiment. This assumption is that, for similar propellers of different diameters, working under similar conditions of slip and at the same translational speed, the forces are proportional to the square of the diameter. Under these conditions the efficiency is independent of the size of the propeller.

Fifteen three-bladed propellers, with a disc area ratio 0.313, were tested. The boss diameter was made $1.56\ \frac{p}{8}$ inches, and the thickness of the blades at the root 9/32. $\frac{d}{8}$ inches in each case. The series was divided into three sets of five propellers, the pitch ratios in the different sets being respectively 0.6, 1.0, and 1.5. Five diameters were tested in each set, namely, 8, 12, 16, 20, and 24 inches. In all cases the power absorbed and given out per unit area decreased with increase in diameter; for the coarse pitch the effect was very

slight, but for the finer pitches the effects were much greater, particularly so in the case of the power given out. So that for the coarser pitches the propellers of greater diameter had lower efficiencies, while for the other pitches the propellers of greater diameter had higher efficiencies.

All the variations were so small that probably most of them were due to experimental errors. The systematic errors mentioned by Taylor were :—

(1) The disturbance in the water not having subsided between the runs, when the larger propellers (20 and 24 inches diameter) were being tested. This effect was small, however.

(2) The greatest error was due to the depth of immersion to the centre of the propeller being the same (16 inches) in each case; so that, while an 8-inch diameter propeller had 12 inches of water over the tips, a 24-inch diameter propeller had only 4 inches of water over the tips.

Both these errors were in the direction of exaggerating the differences between the propellers of different diameters; so that, taking all the factors into consideration, it is probable that, if the results from model tests are applied to a full-size propeller on the square law assumptions, the errors will be very small.

This is a very important conclusion, as the few comparisons that have been made between tests on similar propellers of different sizes and at different speeds in air all tend to show that these assumptions are very close approximations to the truth in air also.

Taylor also determined the thrust deduction for a series of three propellers with different pitches. The thrust deduction is the suction that a propeller exerts on the after-part of a ship, and was determined by measuring the pressure on various planes parallel to the propeller axis. It is not proposed to go into these tests in detail, but Taylor's experiments were in general agreement with those of Froude in giving the thrust deduction as a constant fraction of the thrust.

Durand's Experiments.

The experiments carried out by Durand in the experimental tank at Cornell University[2] dealt only with what have been termed the two chief points, namely, the effect of variation of pitch ratio and blade area.

In all, 49 propellers were tested, seven blade widths being tried for each of seven pitches; the pressure face in each case was a true helical surface of constant pitch. Durand used this pressure face pitch for the determination of the slip ratio, so that the slips given in his results are somewhat higher than they would have been by using the conventional pitch for similar conditions.

The propellers were all four bladed, with elliptical blades, the diameter being 12 inches in all cases. The diameter of the boss was 2.4 inches. The blade thickness was varied as the blade width varied; the thickness on the median line was made practically the same at the tip (being approximately 0.10 inches at one inch from the tip), tapering nearly uniformly to values at the boss, divided evenly between the limits 0.20 and 0.50 inches, being thicker for the wider blades. The values of the blade width ratio (that is, the maximum blade width divided by the radius of the propeller) were divided evenly between the limits 0.20 and 0.80; and the values of the pitch ratio were divided evenly between the limits 0.9 and 2.1.

As Durand has given a short account at the end of his book of the general

[2] "Researches on the Performance of the Screw Propeller" (published by the Carnegie Institution of Washington, 1907).

conclusions to be drawn from his tests, it has been deemed advisable to give these conclusions in his own words :—

(A) The general character of the efficiency curve for varying slip, the pitch ratio and area being constant, is well known . . . , such curves . . . indicate a low value of the efficiency for low values of the slip, rising to a maximum in the neighbourhood of 20 to 25%, and then falling off more gradually for increasing values of slip.

(B) In general there is an indication that the propeller of very small area reaches its efficiency at a relatively high slip, usually 25% or over, while that of large area reaches its best at about 20% and slightly below.

(C) The highest values seem, furthermore, to be only slightly dependent on either area or pitch ratio; by far the larger number of the propellers under test reaching, at some value of the slip, an efficiency close to 70%, with, in general, the best results for fairly high values of the pitch ratio and moderate areas.

(D) In general, propellers of high pitch ratio reach their best efficiency at a somewhat higher slip than those of low pitch ratio.

(E) For low values of the slip, such as 10 to 15%, and low pitch ratios, increase of area is accompanied first by increase in efficiency and then by some falling off; while for higher values of the pitch ratio and low slip there is a general improvement of the efficiency up to some value at which it remains nearly stationary.

(F) For high values of the slip there is, in general, a continuous though slow decrease in efficiency with area through the range under investigation.

(G) Intermediate between these extremes we find, for moderate values of the slip, but slight change of efficiency with area.

(H) For propellers, lying within the more common ranges of pitch ratio and area, and operating at values of the slip between 20 and 30%, it appears that the efficiency will vary but slightly over the midfield range thus indicated, and that any proportions corresponding to operation within these general limits may be freely chosen without fear of any important drop in efficiency below the limits of 68 or 70%.

Experiments on Propellers in Air.

Turning now to a consideration of the experimental work that has been carried out on aerial propellers, it has been found very difficult to co-ordinate the results, as there have been no such systematic series of experiments as have been dealt with in connection with water propellers.

This is probably due to a very large extent to the experimental difficulties that exist, chiefly owing to the high speeds at which the propellers must be run in order to obtain reasonable forces on them. It has been found that the work of the majority of experimenters consists merely of a few unconnected tests, from which very few generalisations can be drawn; for this reason it has only been found possible to touch very briefly on the work of several experimenters.

Turnbull's Experiments.

It is proposed to deal first with the experimental work of W. R. Turnbull,[19] which was published in the form of two articles in the AERONAUTICAL JOURNAL.

[19] "The Laws of Air Screws" (published in the AËRONAUTICAL JOURNAL, October, 1910, and January, 1911).

The chief interest in this work lies in the fact that it is very largely the pioneer experimental work on aerial screw propellers.

The first article deals only with static tests (tests at a fixed point) on propellers; and all subsequent experiments tend to show that these are of very little real value in a determination of the performance of a screw propeller under working conditions. The second article, however, gives an account of some experiments carried out on a moving carriage on a track 300 feet long; the observations were taken during the passage of the carriage over two adjacent stretches of track, each 50 feet long.

The carriage was driven by the propeller by means of a $\frac{3}{4}$ h.p. electric motor, which was also used for measuring the power absorbed. The friction and air resistance of the carriage were much too great, however, so that the slip ratios recorded are all over 50%, which is far in excess of the slip corresponding to maximum efficiency. This renders the actual results somewhat useless, in view of subsequent work.

The efficiency was, however, found to be a maximum for a propeller with a pitch ratio of 1.35. A very great increase in efficiency with increase in the size of the propeller was also found; other experimenters have only found a slight change, and the latest work at the National Physical Laboratory tends to show that any change is very small indeed, and within the limits of experimental accuracy.

Bejeuhr's Experiments.

A few interesting results are given by Paul Bejeuhr[5] in the account of the International Competition at Frankfurt-am-Main in 1909. Propellers up to 5 metres in diameter were mounted on the rear of a car and driven by a 100 h.p petrol motor, at speeds up to 1,800 revs. per minute. The work, being for a competition, was very unsystematic in character, but the following general conclusions were drawn by Bejeuhr:—

From experiments in the wind channel at Göttingen, it was found that the lift on aerofoils of the same sections as the propeller blades was generally zero for negative inclinations of the chord of 2° or 3°. The experiments on the propellers showed, however, that the thrust was zero for negative inclinations of the chords of the blade sections of 6° or 7°, from which the conclusion was drawn that the conditions for propellers are not the same as for aerofoils moving in a straight path. This argument, however, is not correct, as, even if the conditions are the same in the two cases, the angle to give no thrust is not the same as that to give no lift, for the thrust includes a component of the frictional resistance of the aerofoil—*i.e.*, a component of its drag.

Other conclusions were as follows:—

The discharge stream from the propeller is cylindrical and of an area equal to the area swept out by the propeller, the air being drawn in from all sides. The velocity on the suction side of the propeller is practically that in the free atmosphere—*i.e.*, the same whether the propeller is running or not.

In general, propellers with a few narrow, smooth blades are more efficient than those with wide blades, or than those of the snail-shell or turbine type.

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[5] "Luftschrauben" (Denkschrift der ersten Internationalen Luftschiffahrt-Ausstellung (IIa) zu Frankfurt-am-Main, 1909, Vol. II.; Abstract No. 129 for 1911-12).

[NOTE.—Where references are given to Abstracts they refer to the Abstracts in the Reports of the Advisory Committee for Aeronautics].

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Dorand's Experiments.

Some experiments have been carried out by E. Dorand[10] on a track at Chalais-Meudon up to speeds of 12.5 metres per second. Two similar propellers of 2.5 metres and 4.3 metres diameter respectively were tested, and it was found that the forces on the propellers varied as the square of the speed and the square of the diameter for the same values of the ratio $\frac{v}{nd}$ very closely indeed.

This is very interesting, as being a further experimental justification for assuming the square law to hold exactly within practical limits of accuracy. A further series of experiments was then carried out on five propellers of the same diameter and blade area, but with the following values of the pitch ratio :—

$$p = 0.625,\ 0.750,\ 0.874,\ 1.000,\ 1.290.$$

The maximum efficiency (maximum maximorum) was found to occur with a pitch ratio of 1.20, and was about 84%.

Koutchino Experiments.

D. Riabouchinsky[17] has carried out a great number of static tests on propellers at Koutchino, and also some in a current of air. The chief defect with these experiments is that the propellers were so widely different in design from any that are in use in actual practice; the experiments were carried out at air speeds up to 6 metres per second and propeller speeds up to 2,500 revs. per minute. Series of tests were made on similar propellers, but of varying diameter, on propellers with varying pitch, on propellers with varying angle, and also on propellers with a varying number of blades (from one to eleven blades being tried).

Eiffel's[12] Experiments.

Were carried out in a wind channel at air speeds from 5 to 18 metres per second, and propeller speeds from 400 to 1,600 revs. per minute. Propellers up to one metre in diameter were tested.

Eiffel shows that on "square law" assumptions the efficiency should be a function of the slip only; but his experiments indicate that this is not strictly correct, partly due to the square law not holding exactly, but to a greater extent to the deformation of the blades at high speeds.

He shows theoretically, however, that the deformation is the same for similar propellers at the same tip speed; and the results of a few tests on two similar propellers of diameters 2.97 feet and 8.90 feet are shown to agree very well with the square law assumptions, when compared at the same value of the tip speed.

This is but another confirmation of the view that the square law assumptions are sufficiently accurate for practical purposes.

N.P.L. Experiments.

The author[8] has carried out a number of experiments on the whirling arm at the National Physical Laboratory at air speeds up to 3,000 feet per minute

[10] "Etude expérimentale des hélices propulsives" (La Technique Aéronautique, June, 15, 1910; Abstract No. 75 for 1910-11. Also La Technique Aéronautique, November 1, 1910; Abstract No. 102 for 1910-11. Also La Technique Aéronautique, November 1, 1911; Abstract No. 147 for 1911-12).

[17] "Recherches sur les hélices aériennes" (Bulletin de l'Institut Aérodynamique de Koutchino, Vol. I., 1906, and Vol. II., 1909; Abstracts Nos. 38 and 52 for 1909-10).

[12] "Complément de la première Edition de la Resistance de l'Air et l'Aviation" (also 'L'Aerophile," June 15, 1911. Abstracts Nos. 123 and 152 for 1911-12).

[8] Reports of the Advisory Committee for Aeronautics for 1911-12 and 1912-13.

and propeller speeds up to 3,000 revs. per minute; the diameter of all the propellers tested was two feet.

Results of series of tests to determine the effects of variations in blade width and pitch ratio are given in the Report of the Advisory Committee for Aeronautics for 1911-12. Recent improvements in the apparatus used and in the method of testing have led to the suspicion that the absolute values may be in error to a slight extent, but the comparisons between the various propellers are not affected by this The writer feels that he cannot do better than give the general results in a similar manner to that already adopted for those of Durand on water propellers; the results in the two media may then be compared directly. The general results in air are as follows :—

(A) The efficiency-slip curves are of a similar shape to those found for water propellers; the maximum value of the efficiency occurs at a value of the slip between 20 and 30%.

(B) A propeller of width ratio 0.22 reaches its maximum efficiency at a slip of 25%; while a similar propeller of width ratio 0.55 reaches its maximum efficiency at a slip ratio of 20%.

(C) In this series of tests the total variation of efficiency is small. The maximum value of the efficiency in all cases lies between the limits of 71% and 77%, with, in general, the best results for fairly high values of the pitch ratio and moderate areas.

(D) In these tests the slip at which the propellers attain maximum efficiency appears practically independent of the pitch. (Subsequent work shows, however, that, in general, the propellers of higher pitch attain maximum efficiency at a less value of the slip than those of lower pitch.)

(E) At a slip of 20% and a pitch ratio of 0.675, a width ratio of 0.30 gives an efficiency of 65.8%, and a width ratio of 0.47 gives an efficiency of 69%. (As only two values of blade width were tested at this pitch, it is impossible to say whether this is in agreement with the results on water propellers or not.) At a slip of 20% and a pitch ratio of 0.80, a width ratio of 0.30 gives an efficiency of 73.9%, and a width ratio of 0.47 gives an efficiency of 70.8%.

(F) No comparison has been made for slips higher than 30%.

(G) The changes of efficiency with area are fairly small at all places.

(H) This statement for water propellers may be paraphrased, in view of the present series of tests in air, as :—A propeller may be designed of any reasonable form, pitch, and area, lying within the range of common practice, without danger of the efficiency falling outside the limits of, say, 65 and 75%, although it may attain to a value of, say, 77%.

[Recently, a considerably higher efficiency than this has been obtained, the value being approximately 82%.]

A detailed comparison of the above conclusions with the corresponding conclusions drawn by Durand from the results of his tests in water (given on page six) shows to what a remarkable degree the same conclusions apply to the results obtained in the two different media.

Some further results are given in the Report of the Advisory Committee for Aëronautics for 1912-13. The chief general conclusion arrived at is that the "square law" assumptions hold very closely indeed for propellers. A comparison is made between the performances of two propellers differing only by the form of the pressure face; this appears to have very little effect on the results obtained, which is in agreement with wind channel experiments, where the shape of the

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pressure face of an aerofoil has been found to have very little effect on the results obtained from it.

The chief piece of work described, however, is a comparison between actual test results and those obtained by a detailed integration from the results of tests on aerofoils of appropriate sections in the wind channel. This having been referred to at some length in the course of the paper, is merely mentioned here.

It has been found that, although there is a considerable discrepancy between the results obtained in the two cases, it is not of a nature to render the method useless in the hands of a careful designer.

This is only a slight extension of the method initiated by Lanchester and Drzewiecki, which is used at the present time by nearly all designers of aerial propellers.

Summary of Conclusions from Experiments.

As a result of a very careful consideration of all the available experimental information on propellers, the following brief summary of general conclusions has been drawn up :—

(1) The "square law" assumptions are obeyed very closely indeed by propellers, so that the results of model tests and of tests in water may be applied directly to full-size propellers in air.

(2) The method of design usually adopted for air propellers (*i.e.*, considering the blade as an aerofoil) is sufficiently accurate to give good results in the hands of a careful designer.

(3) The best ratio of blade width to radius of propeller appears to be about 0.30. This figure may require considerable modification, as a remarkably high efficiency has been obtained with a four-bladed propeller, having a blade width ratio of only 0.12.

(4) The ratio of pitch to diameter of propeller to give maximum efficiency appears to be anything between 0.8 and 1.4. This is the value that gives the maximum efficiency, but it will probably be found better to use a propeller of less pitch ratio, as the coarse pitch propellers give their maximum efficiency at such a low value of the slip; so that the thrust corresponding to maximum efficiency is too small to enable advantage to be taken of the maximum efficiency on actual aeroplanes.

(5) The propeller should have narrow blade tips, for most efficient working, and to avoid vibration.

(6) Attention should be paid to the shape of the back of the blade; the section of the blade should be such that, if used as an aerofoil, it would give a high value of the ratio of lift to drag.

BRIEF LIST OF PAPERS AND ARTICLES DEALING WITH SCREW PROPELLERS.

NOTE.—Where a reference is given to an Abstract, it is to one of the abstracts of papers given in the Reports of the Advisory Committee for Aëronautics.

Water Propellers.

(1) BILES, PROFESSOR : "The Design and Construction of Ships." Vol. II.
No fresh experimental results are given in the chapters on screw propellers.

(2) DURAND, W. F. : " Researches on the Performance of the Screw Propeller." (Carnegie Institution of Washington, 1907.)

This is a very good paper, and deals exclusively with the effects of changes in blade width and pitch ratio.

(3) FROUDE, R. E. : " Results of Further Model Screw Propeller Experiments." (Proceedings of the Institution of Naval Architects, April 10, 1908.)

This is also a good paper, and gives the results of most of Froude's research work on propellers. It supersedes his previous paper, read before the same Society.

(4) TAYLOR, D. W. : " Resistance of Ships and Screw Propulsion," 1893.

The chapters on the screw propeller only deal with the older work carried out by the Froudes. No fresh experimental work is given.

" Some Recent Experiments at the U.S. Model Basin." (Society of Naval Architects and Marine Engineers, Nov. 17 and 18, 1904.)

" Model Basin Gleanings." (Society of Naval Architects and Marine Engineers. Nov. 22 and 23, 1906.)

These are both very good papers, and cover much the same ground as that of R. E. Froude. On the whole, Taylor's experiments are the fuller and more complete.

Air Propellers.

(5) BEJEUHR, PAUL : " Luftschrauben." (Denkschrift der ersten Internationalen Luftschiffahrt-Ausstellung (IIa.) zu Frankfurt-am-Main, 1909, Vol. II. ; and also Abstract No. 129 for 1911-12.)

The description of the apparatus used and the results in the International Competition are given by Paul Bejeuhr ; also, some static test results obtained in the National Competition are given by C. Eberhardt.

(6) BOLAS, H. : " Notes on Aerial Propellers." (Report of the Advisory Committee for Aeronautics, 1911-12.)

This is a very good paper on the practical application of the blade element theory of Lanchester and Drzewiecki to the actual design of propellers.

(7) BOYER-GUILLON : " Essais des Hélices Aériennes." (Revue de Méchanique Vols. 23 and 24 for 1908 and 1909; also, Abstracts Nos. 31 and 32 for 1909-10.)

These, being all static tests, are of little practical value.

(8) BRAMWELL, F. H. : " Some Notes on the Possible Efficiency of Propellers." " Some Notes on the Effect of Size on the Efficiency and Performance of Propellers."

" Experiments on Model Propellers."

(Report of the Advisory Committee for Aëronautics, 1911-12.) " Experiments on Model Propellers." (Report of the Advisory Committee for Aëronautics, 1912-13.)

The first two papers are theoretical, and the ground covered is indicated by the titles.

The third paper deals with the effects of changes in blade width and pitch ratio.

The fourth paper deals with the effect of changes in the pressure face of a propeller blade, but chiefly consists of a comparison between test results and those obtained by a detailed integration of the forces on elements of the propeller blades.

(9) CROCCO, CAPTAIN: "Les études d'aerodynamique du laboratoire des aérostiers militaires italiens." (La Technique Aéronatique, May 15 and June 1, 1911; also, Abstract No. 120 for 1911-12.)

The propeller work described is chiefly concerned with the question of static tests.

(10) DORAND, E.: "Etude expérimentale des hélices propulsives." (La Technique Aéronatique, June 15, 1910, November 1, 1910, and November 1, 1911; also, Abstracts Nos. 75 and 102 for 1910-11, and No. 147 for 1911-12.)

The first two papers are much the best, and have been referred to previously; the third paper is very disjointed.

(11) DRZEWIECKI, S.: "Des Hélices Aériennes," 1909. "Formules practiques pour le calcul des hélices aériennes." (Comptes Rendus, September 13, 1909; the two papers are also mentioned in Abstracts Nos. 45 and 46 for 1909-10.)

The second paper is similar to the first, but very much condensed. These give no experimental results, but are a clear enunciation of the blade element theory, which is used so extensively by designers of aerial propellers at the present time.

(12) EIFFEL, G.: "Complément de la premiére Edition de la Resistance de l'Air et l'Aviation." "L'Aerophile," June 15, 1911. (These are given in Abstracts Nos. 123 and 152 for 1911-12.)

The experiments described in these two articles are very instructive and well worth study by all interested.

(13) GREENHILL, SIR GEORGE: "Screw Propeller Theory." (Report of the Advisory Committee for Aeronautics, 1911-12.)

This is a reprint of the original paper by W. J. M. Rankine, read before the Institution of Naval Architects in 1865, with a commentary thereon by Sir George Greenhill. This paper is chiefly of interest historically, as the theories advanced have been superseded by later ones.

(14) GUERET, ANDRE: "Notes sur les Hélices Aériennes," 1911.

This book contains some very good chapters on the actual construction of propellers, but no new experimental results are given.

(15) LARARD, C. E., AND BOSWALL, R. O.: "Aerial Propellers and some Test Results." ("Engineering," September 2, 1910, and also Abstract No. 89 for 1910-11.)

Only static tests are described in this paper; these were carried out at the Northampton Institute, Clerkenwell.

(16) LEGRAND, M.: "Mesure de la poussée des Helices en plein vol." ("L'Aérophile," September 15, 1910, and Abstract No. 88 for 1910-11.)

"Etudes expérimentales sur les hélices propulsives aériennes." (Bulletin de la Société d'Encouragement pour l'Industrie National, April, 1911; and Abstract No. 119 for 1911-12.)

These two papers deal chiefly with a description of the apparatus installed on an aeroplane for measuring the thrust on a propeller in flight; very few results are given, however.

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(17) RIABOUCHINSKY, D.: "Recherches sur les hélices aériennes." (Bulletin de l'Institute Aérodynamique de Koutchino, Vol. I., 1906, and Vol. II., 1909; and Abstracts Nos. 38 and 52 for 1909-10.)

These articles describe some interesting results obtained in a wind channel, but they are of little interest to designers, owing to the unconventional shapes adopted for the propeller blades.

(18) SOREAU, RODOLPHE: "L'Hélice aérienne propulsive," 1911. This book deals entirely with the propeller theory developed by Soreau, there being no experimental results.

(19) TURNBULL, W. R.: "The Laws of Air Screws." ("Aëronautical Journal," October, 1910, and January, 1911.)

These two papers are chiefly of interest as being practically the pioneer experimental work on aerial propellers.

DISCUSSION.

Mr. NORTH: I must first thank Mr. Bramwell for his exceedingly interesting lecture. I am extremely interested in his plea for an extension of the blade theory of propeller design.

I have always considered that this method, even though it was based on a broad assumption, and possibly not entirely accurate, nevertheless afforded a very simple method by which propellers might be satisfactorily designed to fulfil any given conditions, provided the requisite experience was available to enable suitable corrections to be made in theoretical results.

Having this in view, it occurred to me some time ago that it would be a very considerable assistance if a few simple formulæ could be obtained which would enable the blade widths, thrust, and consequently efficiency, to be obtained from data of horse-power, diameter, revolutions, and translational velocity, which would make the necessary allowances for the changes in the lift/drag ratios, and lift coefficients at different radii, due to the thickening of the sections towards the boss, as Mr. Bramwell suggested, and also for any curve of blade outline.

These formulæ can be evolved for any given series of aerofoil sections having the same characteristics, but varying chord/thickness ratios, and any given curve of blade outline, and, once the rather cumbersome integration has been carried out, these may be reduced to a form which enables propellers to be designed for any given condition, without recourse to anything more than simple arithmetic, and without the use of any graphical method.

It is, of course, assumed that it is possible to obtain approximate equations respectively for the curves for lift/drag, lift coefficients, and blade outline plotted against radii.

Mr. Bramwell has mentioned that the results of tests on many aerofoils of varying thicknesses have already been published.

The most complete and satisfactory series I have yet seen are those published in the report of the Advisory Committee, 1911-1912, on experiments carried out at the N.P.L. by Mr. Bairstow and Mr. Melvill Jones.

This series gives a range of seven aerofoil sections of the same characteristics, having a thickness/chord ratio varying from .025 to .175. Only the last four of these sections, that is, from .1 to .175, are sufficiently strong to use, and it would be a very great assistance if the series were extended up to .25.

It seems, however, reasonable to suppose that a series of sections might, without very great difficulty, be obtained to give better lift/drag ratios, especially among thicker sections.

I would suggest—and I would be very glad to be corrected in this should I be wrong—that improvements in these sections might be carried out by any one of, or possibly a combination of, the following alterations :—

(1) By thickening up the rear part of a section such that the run at centre part of section is more nearly parallel with the under surface.

(2) To dock the tail of the section—*i.e.*, finishing in a flat, instead of running to a point.

(3) In the sections where the thickness/chord ratio becomes rather high, to make the section convex on the lower surface with a curve having the same characteristics as the top section. All these methods appear to considerably strengthen the section, and at the same time to reduce the rapid change of angle in the run off, which is possibly the cause of inefficiency in thick sections.

If Mr. Bramwell or some other member of the N.P.L. could furnish us with details of tests on some such series of sections, I feel sure that they would be most valuable. In any case, I should be pleased to hear if the lecturer considers that such alterations in the sections are likely to produce good results. When the formulæ are obtained, it is necessary to find constants which shall correct, firstly, errors due to centrifugal force acting on air in contact with propeller : this may be done by tests on propeller in wind tunnel at various angular velocities, and at various translational speeds, to ensure the same angle of attack being presented by the blade.

Secondly, errors due to deformation of the blade.—This could probably be best carried out by tests on a full sized propeller on a whirling arm. In considering the effect of deformation, it is interesting to note that M. Chauvière has found the variations in efficiency to amount to 3 per cent. on similar propellers, due to variations in the quality of the wood, even though this wood was as good as could possibly be obtained.

Thirdly, errors due to the effect of stationary parts of the aeroplane on the propeller. It would seem more satisfactory to carry out these tests on the actual aeroplane, fitted with an apparatus for measuring torque, revolutions, and thrust.

If it were possible to carry out a complete series of experiments on these lines, then the blade element method would allow suitable propellers to be designed for aircraft with a very close degree of accuracy.

Mr. RIACH : There are a number of queries which have occurred to me while endeavouring to obtain particular cases of the equations to which Mr. North has referred.

Some of these have already been answered by Mr. Bramwell during his lecture ; there are, however, one or two points which he has not touched upon.

Firstly, I should like to know whether his experiments have led him to come to any conclusions as to the effect of blade shape on the propeller, apart from the natural effect resulting from surface disposition and aspect ratio.

In view of the very widely varying shapes of most of the well-known propellers, any conclusions of his on this subject would be most valuable.

Secondly, in order to enable the equations of the curves of lift/drag ratios and lift coefficients to be utilised for blades of varying form and size, it is necessary to assume an average aspect ratio when plotting the coefficients. I should be glad to know whether the lecturer considers that a general assumption of an aspect ratio of six would be too inaccurate for general use.

Thirdly, in view of the fact that the average direct drive propeller, such as is, for instance, in use on the Henry Farman, has a peripheral speed of about

550 feet per second, whether he considers it would be advisable to insert a correction for the alteration in the index of V, due to the high velocity?

I would also like to know whether any experiments have been carried out at the National Physical Laboratory to determine the variation in the index of V for velocities exceeding 300 feet per second.

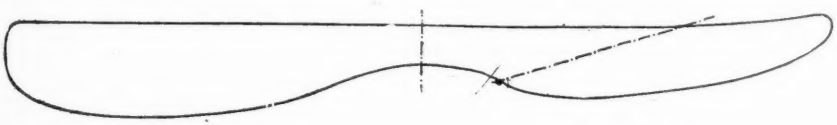
With regard to the choosing of the blade shape, I have found that the equations involved in the general case for propellers of varying section are liable to be very cumbersome, depending, of course, on the simplicity or otherwise of the equations of lift/drag and lift coefficients and blade widths respectively.

With regard to the first two, we can only simplify them by using approximate equations within the desired limit of accuracy. With the blade width equation, however, I have found that the formulæ are greatly simplified by utilising the efficiency curve as the curve of blade widths. From a practical point of view, this has the advantage of ensuring that the blade is widest at its most efficient part, though, of course, it does not necessarily give the most efficient blade.

The chief disadvantage in the light of practice is that, especially in slow running propellers, certain blade widths may lie outside the limits of structural convenience. This may, however, be allowed for by slightly increasing the blade widths towards the tip, and making the necessary corrections to suit this alteration. In most cases the alteration in efficiency amounts to very little.

I should be glad to hear whether Mr. Bramwell considers that this method is likely to prove satisfactory.

Mr. A. F. HOULBERG: My first experiments were carried out with propellers whose blades were set either backwards or forwards to the axis of rotation, as in the Garuda and Twining Propellers. I found that, although those with blades set back gave a far superior efficiency in a static test, there was nothing to choose between the various types under actual flying conditions. In model aeroplane practice a rotational speed of about 800 revs. per minute has been found the most efficient.



UNBENT BLADE. BENT BLADE.

With a propeller as shown in the figure I have obtained better results than with the orthodox carved propellers, although only about three-quarters of the blade is doing any work. This propeller is made from a flat piece of wood cut to shape, and steamed and bent along the dotted line as shown in the figure.

Propellers with somewhat flexible blades seem to suit our rubber motor best, as the pitch is greatest when the torque is greatest, and a more constant speed results.

In general, I have found that propellers with blade sections approximating to that of an aerofoil give the better efficiency.

Mr. A. R. LOW: In the first place Mr. Bramwell is to be congratulated on introducing coefficients of lift and drag which are of zero dimensions, and have therefore the same arithmetical value for any consistent system of units.

The record of the N.P.L. in the matter of units is not a very consistent one.

We find in the Report measurements of velocity in miles per hour, in feet per minute, in feet per second, and against these are tabulated or plotted coefficients of reaction in lbs. per square foot.

Obviously the value of the coefficients is different for the different velocity measures, though the reactions are the same, and speaking from personal experience, it is extremely difficult to make comparisons of results without systematically transforming them all into a consistent rotation. Possibly the desire is to prevent our German competitors from profiting by the work of the National Laboratory! For measurements of force the "Newton" should certainly be adopted.

It is the force which imposes an acceleration of one metre per sec. per sec. on a mass of one kilogram. The horse-power should be dropped and the kilowatt adopted as in electrical engineering.

To pass to definitions of pitch and slip, on which the present speaker has had some discussion with Mr. Bramwell on a previous occasion, reference may be made to the additional notes to the paper on Drzewiecki's method. Soreau and Eiffel are quoted as being in substantial agreement with the present speaker's view.

But the difference is rather one of the line of approach to a certain conclusion than in the conclusion itself.

If Mr. Bramwell's result is sustained, that the experimental mean pitch, or pitch of zero thrust, may be *calculated* by the application of Drzewiecki's method, or by an obvious extension of it, it is then derivable from the geometry of the blade and from wind-tunnel experiments on wing sections similar to blade sections from point to point.

The attitude of the present speaker is that any definition of pitch should be derivable from the geometry of the blade and from standard experiments on wing sections.

If this agrees with actual air-screw experiments, as Mr. Bramwell has shown, the two points of view become identical in result.

With regard to aerodynamical "equivalence" or "similarity" it is to be recognised that Figures 2 and 3 are only illustrations, but Mr. Bramwell must allow it to be said that they are very poor illustrations, as will be shown by exhibiting the curves of the drag coefficients on the same scale. The process that superposes the lift curves will be found to make the drag curves diverge more widely.

At the same time that the illustrations are rejected the main argument as illustrated in Figures 4 and 5 may be accepted as having a certain value.

The really important matter in the present paper is the conclusive demonstration of the analogy between the flow of air past wing sections moving relatively in straight lines, and about air-screw or water-screw blade sections moving spirally.

In the discussion on Drzewiecki's theory last year, Mr. Bramwell seemed to curse the analogy which now, by his admirable experiments, he very completely blesses.

To make a personal point, in suggesting that the form of equations used by the speaker was chosen to give easy integrals, justice is not done either to his mathematical powers or to his engineering sense. It was shown that the form arose from an assumption, based on the interference of the two surfaces of a biplane.

The "Rational Form" keeps the ratio between blade width and path distance constant. A very large variety of blade forms can be constructed leading to easy or at least manageable integrals.

It is to be regretted that the notation of last year's paper has not been adopted. There we have air-screws (and water-screws) divided into right-handed and left-handed, tractor and propeller.

To avoid the hopeless confusion which arises when an attempt is made to distinguish between a tractor propeller and a propeller propeller, the terms sucker propeller and pusher propeller have been attempted, and the term pusher has achieved a certain vogue.

Sucker screw and pusher screw seem quite rational expressions, but pusher propeller seems decidedly a barbarism. Puller screw and pusher screw are also reasonable.

The present writer strongly advocates the adoption of the word screw for the organ of propulsion or traction in every case—air or water.

The paper before us marks a great advance in Mr. Bramwell's previous work (No. 8 in list at end of paper).

Unfortunately there is little money in the matter and this has prevented scientific methods from being adopted in the design room.

The author exaggerates greatly the extent to which such methods are used. The actual method seems generally to have been the manufacture of several hundred standard screws differing by small steps in diameter and pitch, and the rating of the standard list by sheer trial and error.

In conclusion, our recognition is due of the fact that aerodynamics has furnished an indirect experimental method on which to base a screw calculation, which should react in an important way on the calculation of water-screws.

Dr. R. T. GLAZEBROOK complimented Mr. Bramwell on the excellence of his paper, to which he personally had nothing to add. He was glad to hear Mr. Low's complimentary remarks, and agreed with him that one of the reasons why theory had not been further developed by private firms was that there was no money in it. He referred to the verification of theory by experiments such as those undertaken by the N.P.L. Such tests were of very great difficulty. The verification shown in Figs. 6, 7, and 8 was very striking. Dr. Glazebrook mentioned the large number of model experiments that had been made at the laboratory, and the success with which the law of similitude had been established. He referred to the manner in which Froude's law of ship resistance had been founded on model ship experiments. On the question of units, he agreed with what Mr. Low had said, but pointed out that the Laboratory results had to be turned out for the use of practical men, who wanted them in feet per second and miles per hour. They would willingly co-operate, however, with anyone who would help to bring about the general use of a rational system of units, and he asked for Mr. Low's assistance in this direction.

Dr. A. P. THURSTON mentioned the differences in the action of an aerofoil and a propeller blade. He would like to ask the author whether any work had been done at the National Physical Laboratory as to the effect of placing bodies near the propeller, for example, baffle plates.

Mr. BRAMWELL: I am sure that many propeller designers throughout the country will be extremely grateful to Mr. North if he can find time to condense the whole of propeller design into a few simple equations, particularly if these are in a form that can be put into daily use in an ordinary drawing office.

From the results of tests on various forms of aerofoil, it appears that where extra strength or stiffness is required the best results will be obtained by thickening up the pressure face and thus obtaining a convex lower surface. In this year's Report of the Advisory Committee for Aëronautics, some tests on aerofoils of considerable thickness will be found; and also a number of tests on strut sections, the thinnest of which give quite good values for the maximum ratio of lift to drag. The series of tests that I have mentioned in my paper give some information with regard to the effect of centrifugal force on the air in contact with the blades, and also as to the magnitude of the errors introduced by assuming the equivalent aspect ratio of the blade to be the ratio of its length

to its width. It appears that the value of the aspect ratio might with safety be assumed as six; as it is extremely probable that the equivalent of effective aspect ratio of the propeller blade will be somewhat greater than this, and hence actual efficiencies will probably be somewhat higher than those obtained by calculation.

The few comparisons that have been made between tests on full size propellers running at high tip speeds, and those on model propellers, seem to indicate that the index of V can be assumed to be two even up to speeds of 500 feet per second, to the order of accuracy usually required for propeller work.

I do not think that the actual shapes of the plan form of the blades has any appreciable effect on the efficiency of a propeller; although the efficiency will probably depend to some extent on the widths of the blades at the different radii.

With regard to the method of calculation outlined by Mr. Riach, only by actual trial in the drawing office is it possible to decide the details of calculation most suited to the particular type of work required. All that scientific experiment can do is to provide the basis on which various systematic methods of calculation may be built up.

While I am of opinion that non-dimensional coefficients are by far the most convenient for scientific work, yet I am afraid it will be a long time before the average engineer will be content to drop his lbs., slug, foot, and second; and it does not seem quite necessary to add the "Newton," which is 10^5 dynes, to the confusion of units already in existence.

I have plotted the drag curves for the three aerofoils, for which lift curves were given in Figures 2 and 3, and have found that in this particular case, at any rate, the method of plotting against inclinations measured from the "no-lift" lines brings the drag curves into somewhat closer agreement than that of plotting against angles measured from the chords.

I am strongly in favour of retaining the name propeller, whatever its position on the aeroplane. There is no difference in the construction or performance of a propeller built to go before or behind the planes of an aeroplane, and hence it does not seem necessary to have two distinct names.

It seems preferable to say, for example:—

"A biplane with propeller in front";
"A biplane with propeller behind";
or "A biplane with propeller at tail."

No distinction is made between these last two arrangements by the terms:—

"Biplane with pusher screw"
or "Pusher type biplane."

Dr. Thurston has asked about tests on propellers near obstructions. All the tests that have been made at the National Physical Laboratory up to the present have been with the propeller stream as free from obstruction as possible; it is hoped, however, that in the near future some tests may be made on the effect of placing aeroplane bodies, etc., behind a propeller.

Votes of thanks to the Lecturer and Chairman terminated the meeting.

THE EIGHTH MEETING of the FORTY-NINTH SESSION of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, March 4, 1914, at 8.30 p.m. Mr. Mervyn O'Gorman, C.B., M.I.M.E., A.F.Ae.S., presided.

THE RATIONAL DESIGN OF AEROPLANES.

BY ARCHIBALD R. LOW, M.A., ASSOCIATE FELLOW.

The Calculation of Fluid Reaction.

In dealing with the flow of fluids, and the reactions set up on moving surfaces, water and steam turbine engineers have to deal with definite masses of water or steam flowing along channels whose boundary walls are fixed.

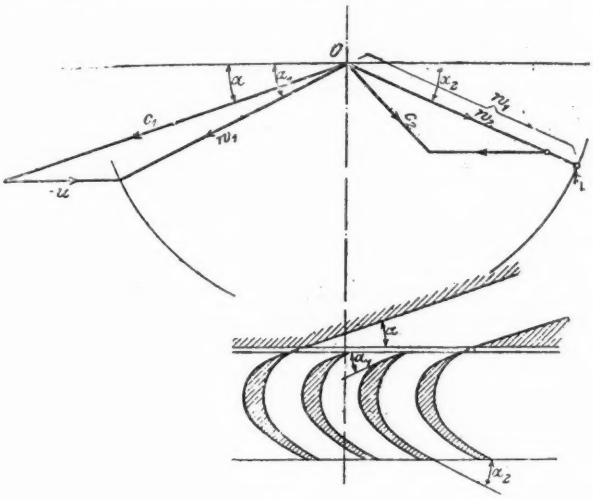


FIG. 1.

Fig. 1 (reproduced from Stodola's Steam Turbines, 3rd edition) shows the nature of the assumption made for the so-called impulse turbine, assumptions which hold to an equal approximation for water turbines, showing nozzle and moving vanes

Reaction turbines present a more complicated state of affairs, which offers a less direct analogy with aeroplanes and airscrews, and which therefore need not be considered here.

In the figure (1) the mass of working fluid delivered per second is the cross section of the nozzle or channels between vanes, at any point, multiplied by the velocity of the fluid relatively to the walls of the channel at that point.

If there were no internal loss of energy in the channels caused by transformation of kinetic energy into heat lost by radiation, through so-called friction or

turbulence, we could apply Newton's law of quantity of motion or momentum, and integrating along the channel, find the total component of reaction in any desired direction, in this case along the line of motion of the moving vanes.

Owing to the loss of energy internally by turbulence or otherwise, the *relative* velocity of the fluid through the vanes diminishes, in a ratio determined by water and steam turbine designers empirically.

This loss is shown by the diminution of w_1 to w_2, w_1 being the relative velocity of entry, w_2 of exit.

The energy lost internally in the vanes per unit mass of working fluid is

$$\frac{w_1^2 - w_2^2}{2g}$$

The total energy transformed is

$$\frac{c_1^2 - c_2^2}{2g}$$

where c_1 is the absolute velocity of delivery from the nozzle, c_2 the final absolute velocity of exit.

Hence the useful work done per unit mass of working fluid is $\frac{c_1^2 - c_2^2}{2g} - \frac{w_1^2 - w_2^2}{2g}$ gravity units.

The determination of $c_1^2 - c_2^2$ in the ideal case where $w_1 = w_2$ is a simple extension of the dynamics of a particle, to a "cloud" of particles, to use Sir George Greenhill's expression, on the assumption that each has a well defined line of flow which has the same mean position between the walls of the channel at every point.

The introduction of the turbulence or fluid friction effect, is *purely experimental*, and the effect on the actual path of a particle of fluid undetermined.

When an attempt was made to extend the analogy to water screws, the whole subject fell at once into the domain of empiricism.

The actual stream lines were each conceived in an entirely erroneous fashion (and still are in modern text books on water screws) or were deliberately slumped into a general backward movement of the so-called slip-stream.

Without denying for a moment that a century's experience and the expenditure of millions of pounds has enabled naval architects to achieve efficiencies probably within 10% of the best obtainable, it is not too much to say that it is from aeronautics that the correct development of a rational theory of air and water screws has come.

Without pursuing this branch of the work further at the moment, it may be remarked that the real cause of failure was that the problem of fluids moving along well defined channels of finite section had given place to the enormously harder problem of the reaction on surfaces of a very large mass of fluid whose relative lines of flow were not determined, to a close approximation, by fixed boundaries.

The first step in hydrodynamics—a comparatively modern branch of applied mathematics—is the solution of the much simpler problem involved in the flow of a very large stream of fluid past a very long and narrow plane barrier.

This problem is, of course, the fundamental problem of flight.

The "cloud" of particles is now a cloud of mutually interfering particles, and this mutual interference renders invalid all attempts to predict the general motion of a large mass of fluid by analogy with the motions of a single particle.

The Relation of Hydrodynamical Theory to Actual Fluid Flow.

From the point of view of giving a sound "outlook" on the nature of fluid flow, the results of hydrodynamics are of great interest and value. When it comes to getting reasonably accurate numerical values we shall see presently that we are thrown back on experimental methods.

Perhaps the most notorious case of misapplication of the dynamics of a cloud of non-interfering particles to fluid flow, is what is commonly called Newton's Law that the reaction of a current of air on a body is proportional to the square of the velocity and to the square of the sine of the angle of incidence of the lifting surface. (On which hypothesis someone has shown that a swallow requires one seventeenth of a horse-power to fly at sixty miles an hour.)

Lord Rayleigh was the first to give a formal value for the reaction on a very long plane barrier in an indefinitely large mass of moving fluid, in accordance with the assumptions of hydrodynamics, including that of mutual interference.

It is sufficient to state that his values at very small angles are about one half of those actually obtained instead of being about one tenth at six degrees or one fortieth at three degrees, as given by the hypothesis of a non-interfering cloud.

As experimental methods now give numerical values, certainly within twenty per cent., and often probably within 10% of the actual reactions, it is seen that the deficiency of 50% given by hydrodynamical theory is quite intolerable to physicists and engineers.

Whether hydrodynamicians are able, by introduction of rotational conditions, to bring their theory more into line with experimental facts, the writer is not sufficiently competent to judge.

But one of the three or four applied mathematicians, of whom this country still boasts, has assured the present writer that progress can be made.

Before coming to experimental results, let us look at three or four examples of the way in which hydrodynamics predicts the general course of experimental results; in a way that would be quite impossible on any preconceived notions derived from particle dynamics.

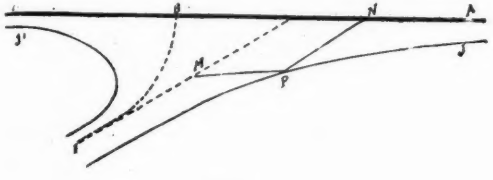


FIG. 2.

Figure 2 shows a stream of fluid impinging on an inclined plane.

The stream divides, part running backwards through an obtuse angle and part running along the plane at an acute angle.

The result may be roughly confirmed experimentally by running a stream from a tap on to a sloping surface, the force of gravity preventing the upper branch of the stream from flowing very far, and bringing it round on either side to join the downward stream.

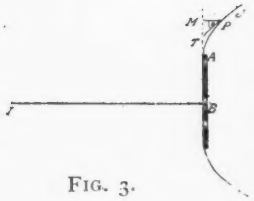


FIG. 3.

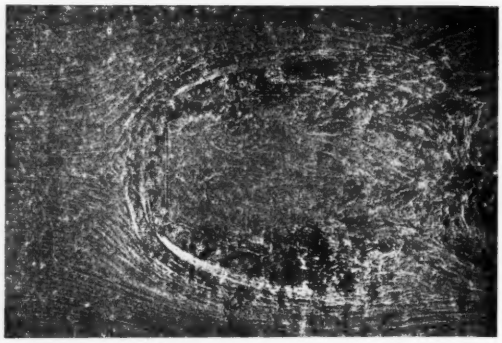


FIG. 4.

Figures 3 and 4 show the strong resemblance between the theoretical boundary line between the stream and the back water, and the experimental boundary line between approximately steady flow and the region of marked turbulence.

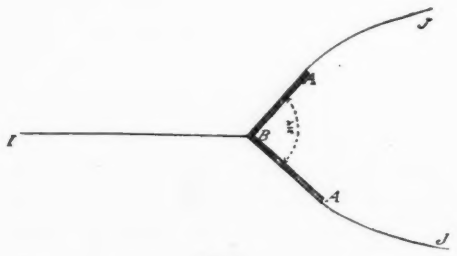


FIG. 5.

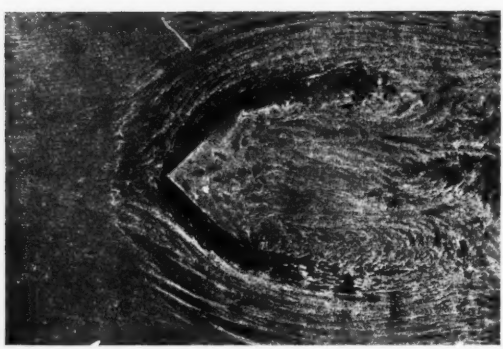


FIG. 6.

Figures 5 and 6 show the same comparison for flow round the pointed buttress of a bridge.

FIG. 7.

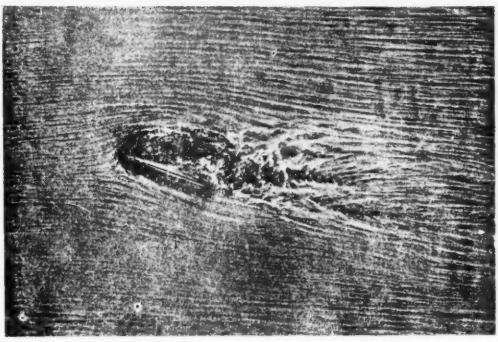


FIG. 8.

Figures 7 and 8 represent the flow round an inclined plane.

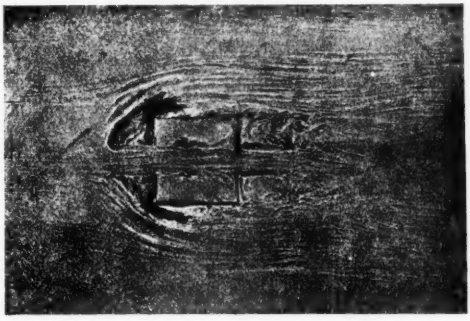


FIG. 9.

Figure 9 is of great interest as representing Eiffel's method of testing in a chamber through which a stream of air flows.

The absence of uniformity of flow illustrates one of the chief experimental difficulties in wind-tunnel work.

The theoretical curves are all taken from Sir George Greenhill's report. The experimental slides from the Bulletin of the Koutchino Laboratory, Part III.

The Basic Conceptions of Aerodynamics.

We are now in a position to draw the conclusion that in aerodynamics we should try to form a correct picture of the lines of flow, and having done so, think in terms of velocities and pressures in accordance with Bernoulli Theorem.

When we try to understand the mechanism of the lifting surface, we can no longer accept the analogy with the steam and water turbine, as so many have done. Instead of the existence of a uniform belt of air receiving a uniform deflection and a definite retardation, we have a series of stream lines round the eddy region, extending theoretically to an infinite distance, each receiving a smaller and smaller deflection with increasing distance from the surface.

There is further a fairly constant and well-defined region of turbulence.

We may however assume that the actual variable infinite stream is replaced by a finite equivalent layer receiving uniform deflection together with a diminution of relative velocity, on the analogy of the turbine. (Fig. 10.)

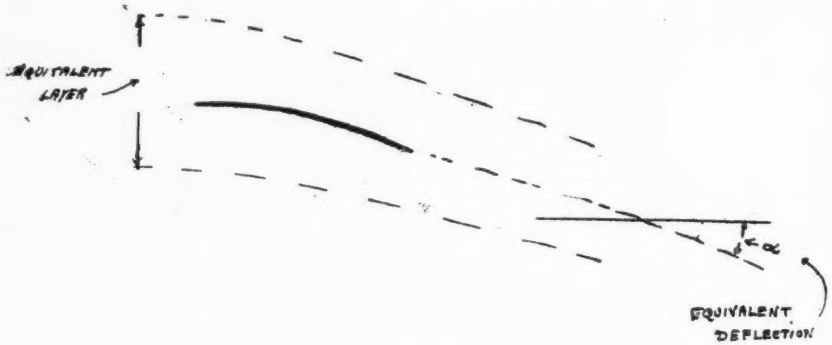


FIG. 10.

The condition of equivalence is that the reaction caused by the change of momentum should be equal in magnitude and direction to the observed reaction, and that the total energy losses should be equal to the thrust work.

It is seen at once that there is an infinite number of ways of assigning the energy losses as between the general deflection of the equivalent stream and the diminution of its relative velocity, corresponding to the general deflection of the infinite stream, and the internal losses in the turbulent region.

There is no experimental method yet known to the writer for separating these losses.

He has suggested (at a lecture at University College) that experiments might be made by injecting colouring or chemical solutions into the centre of the region of turbulence, and testing the concentration from time to time.

Until such methods are developed, the assumptions of an equivalent layer, deflection and loss of energy by reduction of relative velocity, are entirely arbitrary within the limits which assign all the external work to the deflection of the equivalent layer and none to the loss of relative velocity, and *vice versâ*.

Experimental Values of Fluid Reaction.

Turning now to experimental work, most of the results to-day at our disposal are the result of wind-tunnel experiments.

It has been definitely asserted by ill-informed writers that the reaction of still air on a moving surface differs from the reaction of uniformly moving air on a fixed surface in its nature. This assertion denies, of course, the relativity of motion, and so by implication Newton's laws. It is therefore necessary to restate the elementary fact that the discrepancies between wind-tunnel work and measurements on aeroplanes in mid-air, arise from the fact that the walls of the wind-

tunnel reduce the mass of free air, and by their friction disturb the uniformity of flow, not to speak of lack of uniformity of intake.

There is further the effect of the kinematic coefficient of viscosity in creating a certain lack of aerodynamic similitude. The same errors would be introduced if the air in the tunnel were still and the model were carried along it, with the exception of the effect of wall friction in creating lack of uniformity.

We therefore come to wind-tunnel experiment, knowing that many small sources of error exist, and, as far as those outside the National Physical Laboratory are concerned, not knowing whether the average absolute error in applying results to full-sized aeroplanes is 20% or 2%.

Empirical Formulæ.

The results of these experiments are expressed in the form

$$\left.\begin{aligned} R &= KSV^2 \\ Y &= K_y SV^2 \\ X &= K_x SV^2 \end{aligned}\right\}$$ where K, K_y, K_x, are the coefficients of total reaction of lift and of drag.

The assumption that the reaction and its components are proportional to the square of the velocity requires correction on account of the viscosity of air, but as these corrections are refinements beyond the scope of the present paper, it is sufficient to draw attention to the existence of the effect.

Now the object of every experimenter has been to express K_y K_x as functions of the angle i of chord incidence.

It is needless to say that all such functions are empirical, and that on the closeness of the values given by the empirical formulæ to the observed readings depends the usefulness of the formulæ.

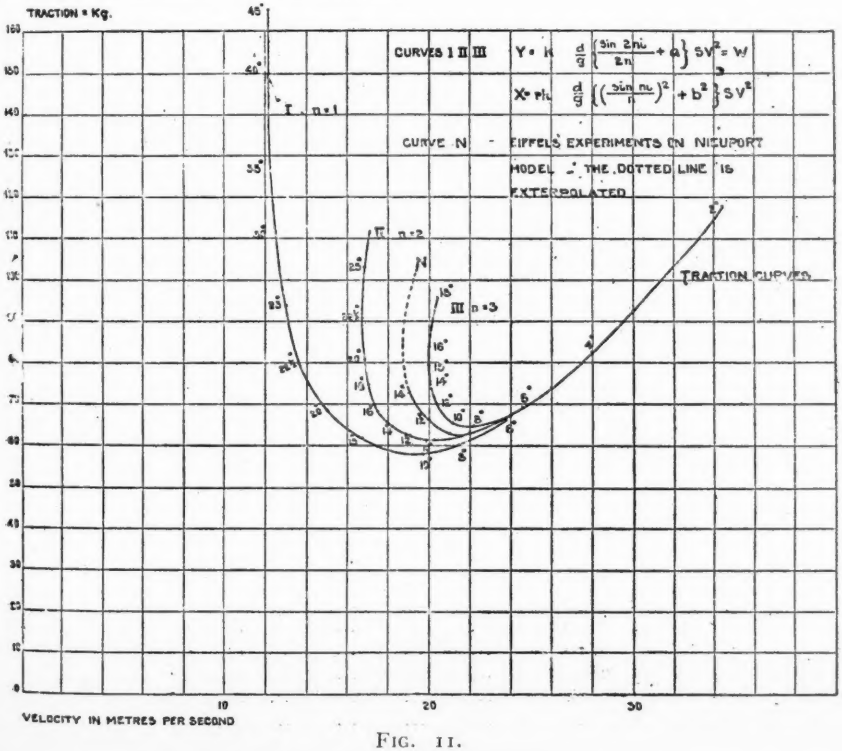


FIG. 11.

An example is shown in Figure 10 of an experimental curve taken from Eiffel's work, and three empirical equations which differ only in substituting multiples of i for i, thus causing the curve of lift to fall off sooner. In this case the experimental curve of lift has its maximum at between 90° ÷ 6 and 90° ÷ 4 and nearly at 90° ÷ 5 or 18° of incidence.

It is also found that the coefficients of lift and drag vary with the aspect ratio and with the camber. So that K_y and K_x are functions of incidence, camber, and aspect ratio.

Another form of the equations is

$$\left.\begin{aligned} R &= k \frac{\Delta}{g} SV^2 \\ Y &= k_y \frac{\Delta}{g} SV^2 \\ X &= k_x \frac{\Delta}{g} SV^2 \end{aligned}\right\} \Delta \text{ is the weight of unit volume of air, } g \text{ the acceleration of gravity.}$$

$$\text{whence } \left.\begin{aligned} K &= k \frac{\Delta}{g} \\ Ky &= k_y \frac{\Delta}{g} \\ Kx &= k_x \frac{\Delta}{g} \end{aligned}\right\}$$

The dimensions of K, K_y, K_x, are those of

$$\frac{R}{SV^2} = \frac{[MLT^{-2}]}{[L^2 L^2 T^{-2}]} = \frac{[M]}{[L^3]}$$

which are the dimensions of density.

The dimensions of k, k_y, k_x, are zero since K and $\frac{\Delta}{g}$ have both the dimensions of density.

Using then the coefficients k, k_y, k_x, of zero dimensions, the following empirical expression follows, fairly closely, a wide range of values for various cambers and aspect ratios.

$k_y = [.53 + .10\,\psi] \sin .6\,a^2\,i + .20\,\psi$

$k_x = .48\,[\sin .3\,a^2\,i]^2 + .05\,\psi + .010$

a^2 is the aspect ratio, so that if the surface is y^2 the sides of the (assumed) rectangular wing are ay and y/a.

ψ is the angle, in radians, between the tangents at the leading and trailing edges taken on the upper surface just before the edges are rounded off on small radii.

These are for surfaces of cylindrical section only.

Surfaces with sections of variable curvature may be compared with the nearest cylindrical surface.

The following table gives values of ψ for different cambers:—

Camber.	Arc in radians.
0	0
1 in 30	.027
1 in 27	.030
1 in 20	.40
1 in 16	.50
1 in 13.5	.059
1 in 12	.066
1 in 8	.098

In order to find the best gliding angle for a lifting surface only, it is necessary to take the ratio k_y/k_x and differentiate with respect to i, a^2, and ψ and equate the partial differential coefficients to zero.

This gives three equations between the three variables and a solution is therefore obtainable if it exists. In practice, however, every engineer will prefer to draw curves of the ratio k_y/k_x and choose the best.

It is found that there is no limit to the increase of the aspect ratio, but that the best camber is about 1-20 or $\psi = .40$.

This best gliding angle for surface only is often assumed to give the best gliding angle for the whole machine.

But here we must assume a constant added to k_x to account for stanchions and bracing which increase with the surface, and another constant to $k_x \frac{\Delta}{g} S$ to represent the head resistance of the body and under-carriage, which are practically independent of the surface, so that the gliding ratio of the whole machine becomes

$$\frac{k_y}{k_x + c_1 + c_2 \div S}$$

The gliding ratio is thus a function of the surface which in turn is a function of the horse-power velocity and total weight, etc.

The variables which we have to introduce are—

Surface
Aspect Ratio.
Camber.
Incidence.
Horse-Power.
Useful Weight.
Engine Weight.
Glider Weight.
Total Weight.

Of which any five may be taken as independent, the other as dependent.

In order to solve formally for the best wing section for the whole machine, we have five variables, and the five partial differential coefficients of k_y/k_x with respect to these variables equated to zero, give the necessary equations for solution.

The lesson to be immediately drawn is that the problem of finding the best camber, chord, span, and incidence for a given machine is not so simple as that of finding the best proportions for wing only.

Trial and error in the design room and tests in the field seem to indicate a camber of about 1 in 15 as a good average.

It is possible, but unlikely, that further investigation will gain an appreciable advantage over the slow and costly process of trial and error.

Of air screws, scarcely anything can be said here, save that recent research by Mr. Bramwell, of the National Physical Laboratory, has confirmed the hypothesis of Drzewiecki, first applied to water screws in 1882, that the analogy of lifting surfaces may be applied to elements of the blades.

An assumed efficiency of seventy per cent. between thrust horse-power and torque horse-power is quite sufficiently accurate for all practical discussion of the general run of phenomena.

XUM

Stress Calculations.

It is now necessary to introduce the question of stress calculations.

Each wing or supporting surface is attached to, and forms part of, a framed structure or girder. This girder is analogous to a girder bridge, whose own weight is unimportant in comparison with a dead load at the centre of the span.

As the weight of the wing itself is supported by the distributed pressure of the wings, no matter how heavy the wings become, and how small the central load, the actual weight of the wing frame itself does not add to the stresses in its members.

Taking into account the special conditions of loading, the established methods of calculating stresses in framed structures are immediately applicable.

Mr. Handley Page read a paper on this subject last year, and it was expected that Mr. Harris Booth would read a paper on the same subject, so that the present writer would have been content to draw attention to the existence of this important special section of aeroplane design. Unfortunately, the gain to the Admiralty of Mr. Booth's technical services, implies the loss to this Society of his published views.

Problems to be Solved.

I shall, therefore state in some detail the problems which present themselves. It is known that in steady flight the root of the wing surface is more heavily loaded than the tip, and that the upper surface is more heavily loaded than the lower. What conventional loading are we to assume?

The whole question of local stresses in gusts is practically untouched.

Again, we have combined compression and buckling in wing spars. Owing to the special form of loading and bracing the fibre stresses from these superposed forces and moments may be approximately equal.

The usual method of analysing these stresses at present is to break up the actual frame into a fictitious pin-joint frame, whose joints coincide with the points of attachment of stanchions and main spars.

To these pin-joints a fictitious continuous beam is supposed to be linked by vertical links; and to this fictitious beam is applied the loading.

In each continuous member the bending moments, points of inflexion and reactions at pin-joints are calculated by the theorem of three moments, usually on the assumption that the points of support are level under all loads, and the reactions vertical. The reactions so calculated are applied to the pin-joint frame and give compressions and tensions from the ordinary stress diagram for pin-joint frames.

The stresses and bending moments so obtained are then superposed and each member is calculated for combined compression and buckling, the free length being taken as the distance between points of inflexion.

Many far-reaching assumptions underlie this method, all of which are approximate, and some of which may be very poor approximations indeed.

Some obvious queries arise.

Are the points of inflexion unaltered when end thrust or end tension is superposed on lateral load?

What is the general nature of the movement of the points of support out of the straight line joining the two end points?

Another type of frame is rendered possible by the use of steel tube and rigid welded sockets for both spars and stanchions.

As the whole frame is rigid, warping is impossible and balancing flaps must be used.

So far as the writer knows no solution of the stresses in this latter frame is available, though it is obviously stronger than a similar frame with pin-joints between the stanchions and the main spars.

To close this section, it is clear that the efforts of designers to lighten frame works have already reduced the weight of the glider for the load it has to carry.

The Coefficient of Glider Weight.

The writer has already, in the Journal, defined a coefficient of glider weight, and has pointed out its importance in determining the maximum useful load that can be lifted or the maximum speed that can be attained with a given useful load.

A good empirical equation for the lower limit of this coefficient to which the best efforts of structural design are tending, is an absolute necessity if general questions of limiting size, speed, useful weight, and desirable horse-power are to be answered accurately.

A whole paper might easily be written on the coefficient of glider weight; space here prevents its further discussion. The symbol used in a previous communication was "k_2."

The Coefficient of Power Plant Weight.

Another coefficient, closely connected with the coefficient of glider weight, is the coefficient of power plant weight, "k_1," which is defined conveniently as the weight of all items which vary with the horse-power.

It is a fair approximation to take a residual constant weight of about fifty kilograms for all horse-powers from 50 upwards, and to represent the additional total weight by the straight line diagram shown in Figure 12 for the bare motor.

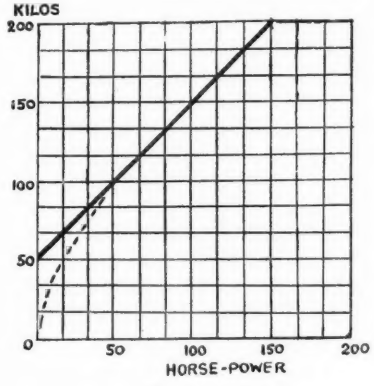


FIG. 12.

This diagram gives $k_1 = \left[\frac{50}{x} + 1\right]$ kilograms where x = h.p.

To this must be added the fuel weight, which is the weight consumed per hour per horse-power multiplied by the horse-power hours of the flight, and the same for cooling water.

Further, an allowance must be made for tankage and radiators, say from 10% to 15% of fuel and water weights.

Further elaborations of the values of k_1 cannot be indulged in here. It should be stated, however, as a matter of common knowledge, that air cooled motors are lighter than water cooled motors per horse-power, but that the consumption of petrol and lubricating oil is greater.

The result is that if power plant weight per horse-power be plotted against hours flight, the straight line of weight for an air cooled motor will in general start much lower for very short flights, and will rise faster, crossing the water cooled line at somewhere between five and fifteen hours' duration.

These figures are only rough and require further very careful discussion by experienced and competent engineers.

If, however, we assume that k_1 and k_2 are known, the former as the simple function of the horse-power x and time t, $k_1 = \left[\frac{c_1}{x} + c_2 + c_3 t \right]$ where c_1 c_2 c_3 are nearly constant coefficients drawn from experience, and if the aerodynamical coefficients k_x k_y are reasonably represented by the empirical formulæ already given we are in a position to investigate the whole range of possible design for any power, useful load, surface, velocity, and range of speed in a quite general manner.

The curves shown in Figure 13 are based on the equations already given to the society.

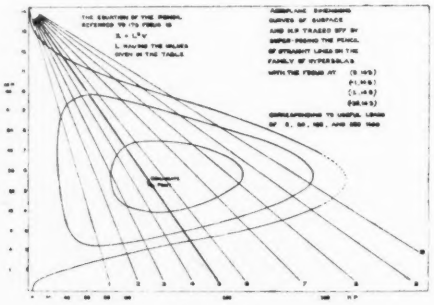


FIG. 13.

They require modification to bring them into closer relation with the actual run of possible design. The present writer hoped to have the modified equations and curves ready in a form which shows substantial agreement with such widely different machines as the BE_2, the Maurice Farman, and the Deperdussin Gordon Bennett racer.

But working single-handed, and having several other lines of work running simultaneously, it has proved impossible to have it ready.

It might be mentioned that the differential equations of looping the loop and nose-diving have also been obtained in soluble form, but here again the work of putting of the results in numerical form is very severe.

Stability.

No mention has yet been made of stability, and till this point it has been assumed that every machine is stable or at least controllable.

There is only time to restate certain problems which are far from generally accepted by aeronautical engineers.

To discuss the problems raised in seeking stability it is necessary to commence with the possibility of equilibrium in flight, for which only the position of the centre of gravity and the reactions on the planes should be known.

If, however, any agency sets up oscillations the whole question of dynamical stability is raised, involving the moments of inertia of the machine A, B, C, the products of inertia, if they exist, D, E, F, about the axes of rotation.

We require to know not only the coefficient of lift and drag for various incidences, but the coefficients of the rotary derivatives, k_{xu} k_{xv} k_{xr}, etc.—to extend Bryan's notation slightly—quantities whose experimental determination has not yet been published.

From these, as we know, Professor Bryan has derived his special forms of Routh's discriminant functions $\mathfrak{A}$ $\mathfrak{B}$ $\mathfrak{C}$ $\mathfrak{D}$ $\mathfrak{E}$ $\mathfrak{H}$.

And Mr. Bairstow has recently shown this society models whose $\mathfrak{A}$ $\mathfrak{B}$ $\mathfrak{C}$ $\mathfrak{D}$ $\mathfrak{E}$ $\mathfrak{H}$ were small negative quantities and which had corresponding types of instability.

The systematic determination of stability coefficients can at present be undertaken only by technical engineers of high competence and reliability. Yet it will not be long before every specification inviting tenders will require the results of stability calculations to be embodied in the tender. The same is already true of nearly every other quantity referred to in this paper.

The object of this paper is achieved if it has brought emphatically to the notice of those interested in aviation what a formidable series of special developments of engineering science must be carried out before it will be possible to say in the design room, "These figures and these drawings will, if translated into wood and canvas, steel and brass, produce an aeroplane whose performance and air-worthy qualities will be thus and thus."

That nation will take the lead whose scientists, and whose technical engineers, and whose works engineers, and whose pilots best understand each other and work together most cordially.

Before the discussion begins I should like to show a few slides.

Figure 14 shows a scale model of an aeroplane in Eiffel's wind chamber.

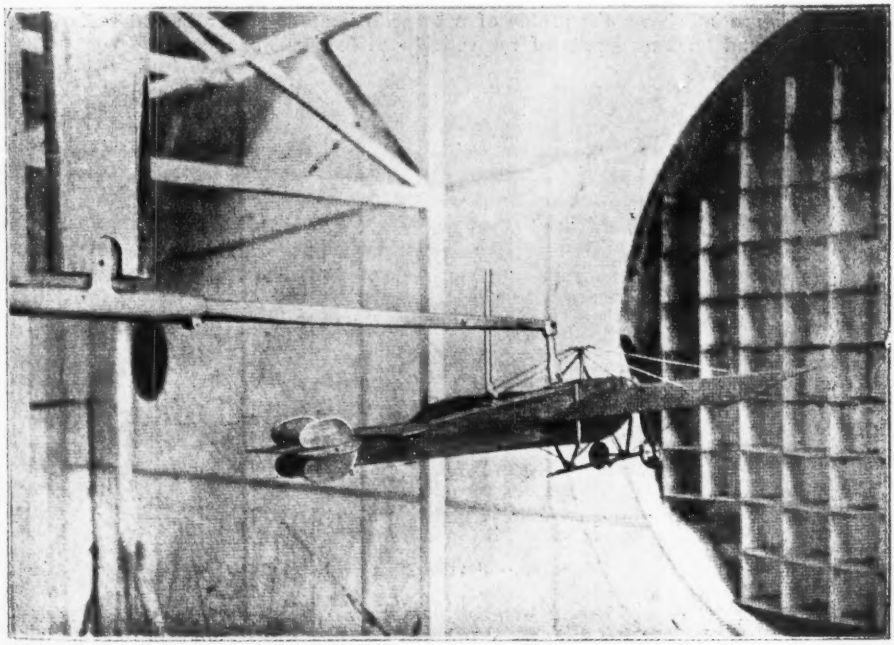
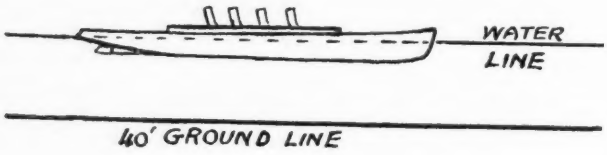


FIG. 14.



240' GROUND LINE

FIG. 15.

TORPEDO-BOAT DESTROYER OF 10' DRAUGHT SHOWN IN WATER 40 FEET AND 240 FEET DEEP.

Figure 15 shows the profile of a torpedo boat destroyer in water showing two ground lines at depths of respectively 40 feet and 240 feet.

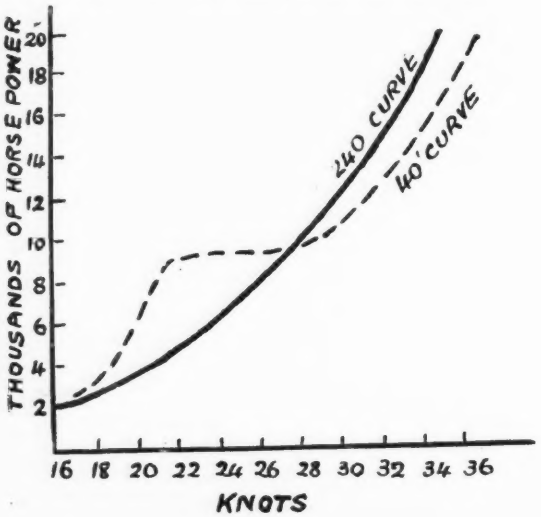


FIG. 16.

CURVES OF POWER DEVELOPED AGAINST SPEED ATTAINED.

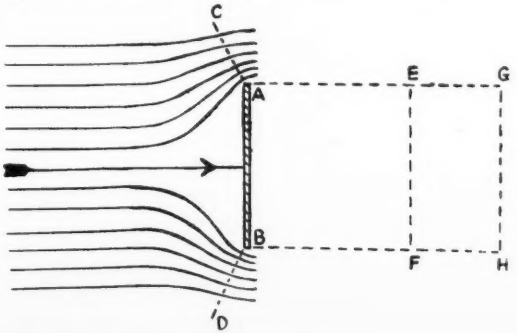
Figure 16 shows curves of power developed against speed attained. The extreme irregularity of the curve in the shallow channel may be compared with the perfect cubical parabola followed by the deep water characteristic. This gives us an unmistakable warning that tests on the scale between model and channel shown in the Eiffel illustration may very probably show great discrepancies with tests where the model is small compared with the air current.

DISCUSSION.

COLONEL DE VILLAMIL: As regards Mr. Low's statement concerning "relativity," I think that his view is correct, or not, according to the *exact meaning* he attaches to "uniformly moving air." If it is to be taken as implying, what is commonly called, *steady motion, and*, if neither the body nor the air are *changing shape*, then it is undoubtedly true. If, however, there be acceleration of the motion of either the body or the fluid, then it is *not true*. The *conditions* must be the same in both cases. In the case of the surface moving in "still air"—at rest, *relatively to the earth*—the fluid is a *static* fluid: in a wind tunnel the air is *not a static fluid*. By "static fluid" I mean one which is neither *changing its velocity* nor *changing its shape*. How far the air in a wind tunnel *changes shape* only experiment could show; but even if we imagine that all the filaments are moving in perfectly straight lines—so that the fluid is *not changing shape*—it is perfectly certain that it is *changing its velocity;* the velocity cannot possibly be the same at *both ends* of the tunnel.

I suppose I must not refer to such "ill-informed writers" as Dubuat and Duchemin—Sir George Stokes bestowed the highest praise on the former, as also did Langley and Zahm on the latter—so I will refer to the mathematicians. It is well known that a body *moving steadily* in an infinite perfect liquid will experience no resistance. If, however, the body be fixed and the same perfect liquid be caused to flow past the body with an *accelerating velocity* the body *will experience resistance*, from what is commonly called the "added mass." The *conditions* are *not the same;* the parallel holds very well to that referred to by Mr. Low. "If the principle of relativity *does not hold good* in any domain of actual life, we must *seek the cause in the material used* and *not in the principle of relativity*" (Paul Carus. "The Principle of Relativity.") All Eiffel's diagrams and wind tunnel photographs show conclusively (to my mind) that the fluid is *not a static fluid*, nor *acting like a static fluid*. I cannot therefore agree with the statement that "*the discrepancies between wind tunnel work and measurements on aeroplanes* arise from the fact that the walls of the wind tunnel reduce the mass of free air": the *conditions* are different, so that the same results could hardly be *expected*.

I was very glad to hear Mr. Low referring to resistance as energy expended —or what we may call wasted. To explain my meaning, let A B represent the



section of a plate exposed to a stream. We will imagine the plate moving in the stagnant liquid. The fluid passing A C and B D is moving *considerably faster* past the plate than the "relative velocity." Some of the potential energy in the fluid has been transformed into *kinetic* energy (according to Bernoulli's law). And this surplus kinetic energy will eventually be converted into heat, and so *wasted*: and the amount of this energy will be a *measure* of the resistance.

Now, can any of this be recuperated? and, if so, how? As is very well known, if we place a box, A E F B, behind the plate, this will recuperate some of this energy: less will be wasted—*ergo, less resistance.* A longer body, A G H B, will recuperate more energy, and so, still further, *reduce the resistance.* To render this possible, it is necessary that the liquid should actually *push the body forward* to some extent: this forward action is what I call "negative resistance." M. Soreau is the only author that I am acquainted with who refers to this action, which he calls "counter-resistance." Both terms are, doubtless, rather barbarous; but as M. Soreau's "counter-resistance" is no improvement on "negative resistance," I prefer to retain the latter, which certainly implies that the "resistance" is acting in a negative direction—*in the wrong way,* in fact.

The same effect is produced when a liquid flows through a tube *the walls of which are wetted.* If the body is *not wetted by the liquid,* the "negative resistance" will apparently not work properly: the *resistance* appears to be *greater* than when the body *is* wetted.

Mr. F. H. Bramwell: I must express my pleasure that Mr. Low has adopted the non-dimensional system of units; several of us have been trying to advance the claims of this splendid method of expressing results for some time, and the addition of Mr. Low to our ranks is extremely gratifying. Mr. Low very properly mentions that the weight of the wings is supported directly by the air pressure and thus does not add to the stresses in their members. I think that a slight addition might, with advantage, be made to this statement. When the machine is running over rough ground, in landing or taking off, the weight of the wings is not supported by the air pressure, and sudden jolts of the machine will give rise to inertia stresses in the wings, which are, of course, dependent on their weight. I do not know if anyone has calculated the stresses likely to be set up by running over a typical piece of rough ground; but it would be a very interesting piece of work, and would settle finally whether these stresses are worth consideration.

The diagram illustrating the size of a model relative to Eiffel's channel, and Mr. Low's remarks thereon, emphasise one rather important point. In tests on models in a wind channel, in which the model is held stationary on some kind of balance, the actual forces on the model can be measured with a high degree of accuracy. So that, even though very elaborate experiments are necessary to determine the scale corrections to be applied, it is generally preferable to determine these once for all and then to use a model sufficiently small in comparison with the size of the channel rather than use a larger model that is appreciably affected by the presence of the walls of the channel or the limits of the moving stream of air.

Mr. F. M. Green said that designers were not allowed to design on the "rational" methods advocated by Mr. Low, as they were compelled to design in a way which he would probably call "irrational."

Modern aeroplanes are designed to have the external resistance as low as possible, with the result that in most cases it would be advisable to have less surface and consequently a heavier wing loading than is generally used. The landing speed in actual practice fixes the area of the supporting surface, subject, of course, to alteration of camber.

The calculation of stresses is certainly a difficult matter, but he did not

think that some of the points raised by Mr. Low were very important; for example, slight want of alignment of the points of support, when taken into consideration, does not materially alter the stresses when using wooden spars.

He had no information on the question of alteration of the point of inflexion of the spar when end thrust or end tension is superposed on a beam uniformly loaded. It has been found in practice, from tests to destruction of actual aeroplanes, that aeroplane spars usually give way under the combined stresses of bending and end load, the end load producing an additional bending moment due to the spar being deflected by the distributed load.

The calculated factor of safety varies somewhat according to the methods of calculation, but much more according to the assumption as to distribution of load. It had given the speaker some encouragement to find that experimental tests are not hopelessly out of agreement with calculations.

Mr. A. R. Low: Colonel de Villamil gives a very vigorous and detailed discussion of what was regarded in the paper from a very simple point of view, namely, that the linear velocity of bodies can be measured only relatively to each other.

The velocity of rotation of a body and the direction of its axis can, however, be measured absolutely even when its rotation is as slow as the earth's, as is shown by the development of the gyroscopic compass.

The non-uniformity of air flow in a tunnel, it is agreed by us both, vitiates direct comparisons, and it is therefore the aim of the National Physical Laboratory and others to eliminate this lack of uniformity so as to reduce the number of corrections necessary.

The other remarks of Colonel de Villamil are of great importance from the hydrodynamical point of view.

Mr. Bramwell and myself are in the happy position of having each converted the other to his point of view.

The important thing is that we are agreed on the desirability of introducing non-dimensional coefficients k k_x k_y instead of Eiffel's coefficients of the dimensions of density K K_x K_y.

I hope that Messrs. Bairstow and Bramwell will adopt the suggestion that *coefficients* of the forces which are the differential coefficients of linear and angular displacement from steady non-rotational motion should be expressed likewise by means of the non-dimensional coefficients k_{xu}, k_{xv}, k_{xr}, etc.

The first suffix denotes the direction of the force or the axis of the couple; the second suffix denotes the direction of the linear speed variation or the axis of the rotational displacement from the position of steady flying.

Mr. Bramwell's remarks about the inertia of heavy wings or wing spars are, of course, in accordance with experience.

I remember personally the shock I received, after flying a biplane with light flaps, when I first piloted a large monoplane with heavy unbalanced rear spars.

While rolling, the lever was forcibly dashed from side to side of the body and no muscular effort could reduce this disconcerting motion appreciably.

Mr. Bramwell's further remarks on wind-tunnel work I commend to Colonel de Villamil.

Mr. Green emphasises the still vast chasm that yawns between the higher developments of theory and the every-day practice which is forced on designers by the necessity of going ahead.

But so far from calling his methods and those of his colleagues at the Royal Aircraft Factory irrational, I should say from long personal knowledge

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of what has been done at Farnborough that they are by far the most rational of any staff in the world.

Wherever systematic measurement of results is carried out with a view to improving subsequent types, there, in my humble estimation, we have the fundamental and distinguishing feature of rational design.

By far the most successful private constructor, who has recently carried off a most important international trophy, made a very amusing speech at a celebration of his success, from which the audience might have gathered that a keen sense of humour was the most important thing a designer should have.

But in the dim and distant days when that pilot was preparing for his successful attack on the Baron de Forest Prize, I used to see him day in, day out, flying in all sorts of weather at all sorts of speeds, measuring systematically weights, petrol consumption, and speeds, and observing systematically every phenomenon of the air.

I would only warn those of our members who are budding constructors, that they must not take too literally the apparent under-estimation by such successful designers of their powers of systematic observation and of correct inference.

With regard to stress calculations in particular, I have had my own calculated stresses in a certain design questioned by the Royal Aircraft Factory, and have had to admit errors (due, of course, to an assistant!)

More recently I have heard that another Government Department has cancelled a considerable order to a firm on the grounds that the stress calculations, on examination, were found to be incorrect.

Finally, if one takes the analogy of steam engineering or electrical engineering, one may well believe that a proportional development of aviation will require, equally, the highest and most extensive possible development of technical research and method.

Votes of thanks to the Lecturer and Chairman terminated the meeting.

ANNUAL GENERAL MEETING.

THE ANNUAL GENERAL MEETING of the Aëronautical Society of Great Britain was held on Wednesday, March 18, at 8 p.m., at the Royal United Service Institution, Whitehall, S.W. The Chairman of Council, Major-General R. M. Ruck, C.B., R.E. (retired), presided.

THE CHAIRMAN: The business of this meeting is set forth in Rule 37. It is to receive and approve the report of the Council on the state of the Society, and the Balance Sheet of Aerial Science, Ltd. To discuss and determine such questions as may be proposed by Council or voters (in this case none) and to fill the vacancies on the Council for the ensuing year.

The Chairman then read the Council's Report for 1913-14:—

The results of the Society's work during the past year have been in some important respects the most satisfactory during recent years. The acknowledged position of the Society has been fully maintained, and a sound foundation laid for future progress in the direction of developing the technical side.

Meetings.—The technical papers read at the fortnightly meetings continue to be of the high standard previously set. The tribute to the national and scientific importance of the Society's work paid by Lord Sydenham, who presided at Lieut.-Colonel Sykes' lecture on 4th February last, should be a source of much gratification to Members, coming as it did from so eminent an authority on Imperial Defence. The Council desire to acknowledge the kindness of Sir John Thornycroft, LL.D., F.R.S., Professor J. E. Petavel, F.R.S., A.F.Ae.S., Dr. H. N. Dickson, D.Sc. (President of the Royal Meteorological Society), Sir Richard Atkin, Lord Montagu of Beaulieu, and Sir Alfred Keogh, K.C.B., in presiding at the fortnightly meetings.

Wilbur Wright Memorial Lecture.—The first Wilbur Wright Memorial Lecture was read by Mr. Horace Darwin, F.R.S., F.Ae.S., on 21st May, 1913, before a large and representative audience. The meeting and the dinner which preceded it formed a fitting tribute to the memory of our late distinguished Member.

The Memorial Fund is in an entirely satisfactory position, and thanks to the efforts of Mr. Griffith Brewer will become larger in the future. In December last £550 4% Stock (a Trustee security) was purchased on advantageous terms. The interest from this investment, apart from further accretions to the Fund, guarantees an annual premium of £21 for the lecture.

The second Wilbur Wright Memorial Lecture will be delivered by Dr. R. T Glazebrook, F.R.S., F.Ae.S., on May 20th, 1914.

Public Safety and Accidents Investigation Committee.—This Committee, on which the Society is well represented, has done valuable work during the past year. The Chairman, Colonel H. C. Holden, F.R.S., F.Ae.S., has kindly consented to give to the Society a summary of the Committee's work and the conclusions to be drawn therefrom, in his paper on "Lessons Accidents have Taught."

Fellows Elections.—The first and second elections of Fellows took place in September, 1913, and resulted in the following representative names being placed upon the roll:—Horace Darwin, F.R.S., W. H. Dines, F.R.S., J. W. Dunne, Dr. R. T. Glazebrook, F.R.S., Sir George Greenhill, F.R.S., Col. H. C. Holden, F.R.S., Alec Ogilvie, and Dr. W. N. Shaw, F.R.S.

Associate Fellows Election.—The third election of Associate Fellows took place in June, 1913, with the result that twenty names were added to the roll, which now numbers 59.

Secretaryship.—In view of the improved financial position of the Society, which it was evident would result from the year's working, the Council at the end of last year decided that a salary could be attached to the post that would make it possible to invite applications for whole-time service. After consideration of the candidates (thirty) who applied, it was decided to appoint Mr. Bertram G. Cooper to be Secretary for two years, from the 1st February, 1914.

Educational Lectures.—At the request of the Working Men's Club and Institute Union, the Secretary has delivered on behalf of the Society further educational lectures to Working Men's Clubs, in Wiltshire, Yorkshire, and Lambeth. The interest taken in these lectures is shown by the fact that at Swindon the Chairman of the Local Educational Committee took the Chair and the attendance numbered 1,200.

The Council are anxious to extend the educational work of the Society, and have welcomed the opportunity of co-operating with the Society of Motor Manufacturers and Traders, as mentioned below, by giving a series of educational lectures at the Aero Show.

Further lectures will be organised as occasion offers.

Library.—One hundred and five books and pamphlets have been added to the Library during the year under report, and the collection of lantern slides has been considerably increased. With a view to enlarging the usefulness of the Library the Council have approved the recommendations, made by the Library Committee in a Report dated September last, for its reorganisation. As soon as the necessary time can be given to the work this will be proceeded with. The Council are much indebted to Mr. H. F. Lloyd for his assistance in the work of the Library Committee.

Ghent International Exhibition, 1913.—The Society was able to assist the British Section of the above Exhibition by the loan of the Pilcher Glider and a collection of old prints, photographs, and posters.

Aero Show.—At the Aero, Motor Boat, and Marine Exhibition at Olympia (March 16th-25th), the Society has, by arrangement with the Society of Motor Manufacturers and Traders, organised a series of short lectures, to be delivered each day during the Exhibition. The object of the lectures is to give to the public a general understanding of the scientific principles of aeronautics.

Full details of the lectures appear in the official catalogue and notices.

The Society will have a small Office for inquiries, etc., on Stand 20.

Technical Terms Committee.—The need having become pressing for an extension of the preliminary list of Technical Terms published by the Society in 1910, a Committee has been appointed to take up the question.

Research Committee.—It has not been possible to organise experimental work during the year, but valuable reports to the Committee were made by Messrs. Harris Booth and B. Melvill Jones (The Measurement of Wind Velocity), Messrs. Mann and Brimelow (Tests on Eyebolts), and Messrs. J. C. Stewart and L. Reeves (Tensile Tests on Flexible Steel Cable and Cable Ends). (The two latter series of tests were initiated by Dr. Thurston.) The paper on "Propellers," read by Mr. Bramwell on February 18th, 1914, was originally prepared as a communication to the Research Committee.

Students' Section.—A thorough inquiry is now being made into the possibility of organising the Students' Section of this Society, which now numbers twenty-two, into groups of local associations on the lines of those of other

Technical Societies. A proposed Students' Prize Fund is under consideration and means are also being considered for associating with the Society provincial groups having scientific aims.

Co-operation with the Royal Meteorological Society.—The Royal Meteorological Society were the guests of the Society on the occasion of Dr. Shaw's lecture on "Wind Gusts and the Structure of Aerial Disturbances" (January 7th, 1914), and a successful combined discussion took place. Joint meetings of the two bodies will, it is hoped, become part of the annual session. Arising out of a suggestion made during the meeting, arrangements are now being considered for the formation of a joint Committee of the two Societies, having as its primary objects the utilisation by each Society of the special knowledge of the other for the advancement of both sciences.

Meteorological Conference at Edinburgh, September, 1914.—In view of the absence from England this year of the British Association, it has been arranged to hold a Conference of observers and students of meteorology and allied subjects at Edinburgh in September next. Major-General R. M. Ruck, C.B., R.E. (ret.), Chairman of Council, and the Secretary are representing the Society on the Committee (presided over by Dr. W. N. Shaw, Sc.D., LL.D., F.R.S., F.Ae.S.) who are organising the Conference. The Council would be glad to hear from any Member wishing to take part in the Conference or to contribute a paper thereto.

Finance.—The financial position of the Society is now much more satisfactory than for many years. Although there has been no abnormal influx of Members into any of the grades of the Society, 50 new members have been elected during the year. The subscription income has therefore increased, and owing to considerable economies which have been effected in the expenditure, the balance of income over expenditure during the year 1913 was £64 and the total cash credit of the Society on 31st December was approximately £180.

THE CHAIRMAN: A copy of the accounts has been circulated to every member, and I shall now be glad if anyone who desires to do so will comment on the Report or the accounts.

There being no remarks the Chairman put the Report and Accounts to the Meeting, and they were adopted *nem. con.*

The Chairman then explained the position with regard to the election of Council Members: Of the eight Members due to retire, two (Colonel J. E. Capper and Mr. J. W. Dunne) have resigned in advance of the election. To fill one of these vacancies the Council has co-opted Mr. Harris Booth, and the other vacancy is under consideration. As was the case last year, no additional nominations for Council have been received, and it is now my duty to ask some Member to propose that the following gentlemen be re-elected to the Council: Mr. Harris Booth, Mr. J. H. Ledeboer, Mr. F. K. McClean, Major-General R. M. Ruck, C.B., Lieutenant-Colonel F. H. Sykes, Dr. T. E. Stanton, D.Sc., Dr. R. Mullineux Walmsley, D.Sc.

The motion was proposed by Mr. Griffith Brewer, seconded by Mr. Mervyn O'Gorman, put to the Meeting and carried.

On the proposition of Mr. Mervyn O'Gorman a hearty vote of thanks was passed to General Ruck for presiding, and the Meeting then terminated.

THE DESIGN OF FLOATS.

It is perhaps not remarkable that the floats fitted to many hydro-aeroplanes are such as would move a naval architect to derision. The wide range of knowledge demanded from aircraft designers often cannot be adequately compassed by a busy man in the time allowed him, and the tendency to allow the design of what appears to be a detail, rather subsidiary to the working out of the aerodynamical qualities of a machine, to be scamped, is easily understood. Nevertheless, the hastening of the evolution of a process of real design of aircraft, as opposed to the haphazard bringing together of parts not related to each other by any logical scheme as a whole, demands that the greatest advantage be taken of the work of all kindred sciences where it can be utilised. There is no doubt that the work of naval architects can be usefully considered in the design of seaplanes, and the formulæ printed below have been very kindly presented to the Society, for the use of his fellow members, by a very well-known and skilled designer of sailing and motor boats. It is to be hoped that those designers of waterplanes who have been using a very nondescript form of the "C" type of float will draw the obvious moral from the data now given.—EDITOR.

FORMULÆ FOR ASCERTAINING RESISTANCE AND EFFECTIVE HORSE-POWER (OR TOW-ROPE HORSE-POWER) OF SKIMMING BOATS, OR HYDRO-AEROPLANE FLOATS.

BY LINTON HOPE.

The following formulæ have been devised for ascertaining the resistance and power of skimming boats, but they are equally applicable to the floats of hydro-aeroplanes, although all the examples given are of skimming boats varying from 15 to 40ft. in length, the speeds varying from just under 28 knots up to 51 knots.

It will, of course, be readily understood that the power required to overcome the resistance caused by the floats on hydro-aeroplanes is the Effective Horse-Power (E.H.P.), which is the same as the H.P. required to tow a float at any given speed; while the formula for resistance only, does not take speed into account except in the ratio of $\frac{V}{\sqrt{L}}$

In the examples given it will be seen that the variation between R, as ascertained from actual trials by the ordinary ship formula of $\frac{\text{E.H.P.}}{.0030707V}$ and r, ascertained by the formula of $\frac{W.\left(\frac{V}{\sqrt{L}}\right)^{1.4}}{K}$, is very small; while, in the case of E.H.P. from trials, and e.h.p. by the formula $W.V.\frac{\left(\frac{V}{\sqrt{L}}\right)^{1.4}}{C}$ the maximum error—if we except example No. 5 (which is a known bad boat)—is only 0.39%, and the average for the other nine examples is 0.2% for the A type, .04% for B, and 0% for C type.

In these formulæ W = total running weight in lbs., V = speed in knots, L = length-over-all in feet, while E.H.P. is .475 B.H.P. except in Nos. 1 and 4 (which are good), and in No. 5 (which is bad), but the greatest variation is only from .47 to .48 B.H.P.

The constant C for ascertaining e.h.p., is obtained by multiplying 138860 (for single-step type) by the ratio of $\frac{B}{L}$; for the multistep type, $C = 128780\frac{B}{L}$ while for the stepless type, such as the ordinary hydro-aeroplane float, $C = 112700\frac{B}{L}$.

In the same way, the constant K for resistance only, is $425\frac{B}{L}$ for the single step type, $395\frac{B}{L}$ for the multistep type, and $346\frac{B}{L}$ for the stepless type.

SINGLE-STEP SKIMMERS, TYPE (A).

(A)

No.	L (o.a.).	W. (lbs.)	$\frac{B}{L}$	E.H.P.	V (knots)	A Type $C = 138860\frac{B}{L}$	e.h.p. $= \frac{WV\left(\frac{V}{\sqrt{L}}\right)1.4}{C}$	Error %	$R = \frac{E.H.P.}{.00307 \sigma V^3}$	$\frac{W\left(\frac{V}{\sqrt{L}}\right)1.4}{K}$	$K = 425\frac{B}{L}$
1.	26.00′	2650	.23078	26.29	28.50	32050	26.22	0.26%	300.36	300.50	98.09
2.	22.50′	1820	.22222	19.00	27.80	30864	19.04	0.21%	222.53	223.75	94.44
3.	22.50′	1817	.22222	25.18	30.96	30864	25.93	0.30%	264.28	265.95	94.44
4.	26.00′	3100	.23078	52.32	35.56	32050	52.30	0.00%	479.08	480.58	98.09
5.	28.00′	3023	.21428	51.23	35.00	29760	50.07	2.26%	476.65	467.50	91.07
6.	25.00′	2350	.22000	50.03	38.00	30557	50.01	0.04%	428.84	430.08	93.50
7.	33.00′	5200	.19848	127.30	41.80	27566	126.90	0.31%	991.78	990.81	84.35
8.	40.00′	11700	.22500	338.40	50.00	31250	338.40	0.00%	2204.06	2211.53	95.63
9.	15.00′	672	.22666	23.75	41.00	31480	23.81	0.25%	188.60	189.84	96.33
10.	23.00′	3360	.21800	154.38	51.00	30279	154.97	0.39%	973.36	971.11	92.65

Average 0.20% without No. 5.

(B) MULTI-STEP, TYPE (B). $C = 128780\frac{B}{L}$ $K = 395\frac{B}{L}$

1.	26.24′	1869	.20000	31.35	32.50	25756	31.35	0.00%	314.14	316.55	79.00
2.	40.00′	12000	.22500	361.00	49.25	28986	360.76	0.07%	2387.07	2382.76	88.88

Average 0.04%.

(C) STEPLESS, TYPE (C). $C = 11568\frac{B}{L}$ $K = 355\frac{B}{L}$

1.	21.00′	1339	.21430	19.00	28.00	24790	19.06	0.32%	220.96	222.08	76.00
2.	15.00′	666	.23120	23.75	38.00	26220	23.74	0.01%	203.54	204.20	80.46
3.	21.00′	1550	.20619	21.38	27.30	23850	21.36	0.09%	255.31	255.12	72.20

FORMULÆ.

C = constant for E.H.P. (tow-rope H.P.).

$C = 138860\frac{B}{L}$ for A type, $128780\frac{B}{L}$ for B type and $112700\frac{B}{L}$ for C. type.

K = constant for resistance.

$K = 425\frac{B}{L}$ for A type, $395\frac{B}{L}$ for B type and $346\frac{B}{L}$ for C type.

From trials.

$$\text{Resistance} = R = \frac{\text{E.H.P.}}{.00307\,V} \text{ or by formula } r = \frac{W\left(\frac{V}{\sqrt{L}}\right)^{1.4}}{K}$$

From trials.

E.H.P. (or tow-rope H.P.) = .470 to .480 B.H.P. by formula,

$$\text{e.h.p.} = \frac{WV\left(\frac{V}{\sqrt{L}}\right)^{1.4}}{C}$$

NOTES.—No. 5 (A) is a known bad boat, as she has the engine and propeller taken out of No. 4 (A) ("Miranda IV") and although 2ft. longer for the same beam, and 77lbs. less weight, is .56 knots slower, whereas she should be a trifle faster. No. 1 (A) is "Miranda IV." with four cylinders instead of eight, as in No. 4 (A). No. 2 (in the C type) is No. 9 (A type) with step removed, all else remaining the same. Nos. 2 and 3 (A) are identical designs with different powers and makes of motor.

TESTS FOR AEROPLANES OF PRIVATE DESIGN.

[The official document reproduced below, although in the nature of things destined soon to be out-of-date, is placed on record here, not because in a year or two it may read like Chaucer (to adapt Colonel Sykes' happy expression, when speaking of the instructions issued in 1910, at the meeting of the 4th February), but because it is the first published attempt at official standardising of aircraft requirements. It is not proposed to subject it here to any detailed criticism, but it may be pointed out that the "minimum factor of safety of 6," which is required, is not a factor of safety at all of anything like 6 in the ordinary engineering sense, but merely denotes the margin between the minimum and maximum stresses which are anticipated.—EDITOR.]

1. The Chief Inspector of Military Aeronautics is prepared, on the request of an aeroplane constructor, to put an aeroplane through the ordinary military acceptance test under the following conditions:—

(i.) The test consists of examination of workmanship and materials, speed test, fast and slow, climbing, weight of load carried, rolling test, and one hour's flight. The constructor must supply the pilot and passenger. For purposes of calculation weights of pilot and passenger will be 160lbs. each.

(ii.) Stress diagrams in duplicate for the aeroplane must be sent with or before the machine. A minimum factor of safety of 6 throughout is essential.

(iii.) No machine will be tested for military purposes unless it fulfils the conditions of one of the types used for military purposes. These are given in attached table.

(iv.) The constructor, when applying to have his machine tested, should state his reasonable expectation of the performances of the machine.

(v.) Aeroplanes submitted for test must be put through the whole of the tests unless damaged before their completion, or unless the Chief Inspector considers that the tests should be stopped for reasons of safety.

PERFORMANCES REQUIRED FROM VARIOUS MILITARY TYPES.

	Light Scout.	Reconnaissance Aeroplane (*a*).	Reconnaissance Aeroplane (*b*).	Fighting Aeroplane (*a*).	Fighting Aeroplane (*b*).
Tankage to give an endurance of ...	300 miles.	300 miles.	200 miles.	200 miles.	300 miles.
To carry	Pilot only	Pilot and observer, plus 80lbs. for wireless equipment.	Pilot and observer, plus 80lbs. for wireless equipment.	Pilot and gunner, plus 300lbs. for gun and ammunition.	Pilot and gunner, plus 100lbs.
Range of Speed ...	50 to 85 m.p.h.	45 to 75 m.p.h.	35 to 60 m.p.h.	45 to 65 m.p.h.	45 to 75 m.p.h.
To climb 3,500 feet in...	5 minutes.	7 minutes.	10 minutes.	10 minutes.	8 minutes.
Miscellaneous qualities	Capable of being started by the Pilot single-handed.	—	To land over a 30ft. vertical obstacle and pull up within a distance of 100yds. from that obstacle, the wind not being more than 15 m.p.h. A very good view essential.	A clear field of fire in every direction up to 30° from the line of flight.	A clear field of fire in every direction up to 30° from the line of flight.

Instructional aeroplanes with an endurance of 150 miles will also be tested under special conditions; safety and ease of handling will be of first importance in this type.

2. The Chief Inspector of Military Aeronautics is also prepared to examine and test aeroplanes which may be designed not for purely military purposes, but to demonstrate some practical or theoretical improvement in design or construction. The tests imposed in such cases will be at the discretion of the Chief Inspector.

3. Results of any test will be supplied to the constructor by the Chief Inspector, and will be kept secret, if desired by the constructor. Should the constructor wish to publish the result of the test, it is to be understood that the result should be published complete. Should only part of any report of the test be published, the Chief Inspector reserves the right to publish it in full.

4. The satisfactory performance of the tests laid down in paragraph 1 does not constitute a guarantee that the aeroplane in question will be purchased by Government.

5. These tests may be altered from time to time; notice will be given as early as possible of any alteration.

War Office,
February, 1914.

BRITISH FATALITIES.

Date—1913-14.	Name.	Nationality.	Machine.	Place.
Oct. 3, 1913.	*Major Merrick.	English.	Short (2).	Upavon.
March 10.	Capt. C. Downer.	English.	B.E. (2).	Upavon.
March 11.	Capt. C. R. W. Allen. / Lieut. J. E. G. Burroughs (P.)	English.	B.E. (2).	Netheravon.
March 19.	Lieutenant H. Treeby.	English.	M. Farman (2).	Upavon.
April 8.	Sergt. E. Deane.	English.	Bristol (2).	Brooklands.

*Inadvertently omitted from last issue. (P.) Passenger. (2) Biplane.

NEW BOOKS AND PUBLICATIONS.

Agenda du Mecanicien. A. Nachtergal. Bruxelles: A. Bieleveld. 1914. pp. 254. Figs. 3frs. 50.

Beitrag zur Berechnung der Luftschrauben. Dipl. Ing. C. Dornier. Berlin: J. Springer. 1912. pp. 108. Figs.

Warnende Stimmen in Bezug auf Zeppelin-Ballons. V. Silber. Wien: Kommissionsverlag von L. Seidel. 1914. pp. 34.

*__Flying: Some Practical Experiences.__ G. Hamel and C. C. Turner. London: Longmans Green and Co. 1914. pp. 341. Illus. 12s. 6d. net.

Die Gesetze des Wasser und Luftwiderstandes und ihre Anwendung in der Flugtechnik. Dr. O. Martienssen. Berlin: J. Springer. 1913. pp. 131. Figs. M.5.40.

*__Taschenbuch der Luftflotten, 1914.__ F. Rasch und W. Hormel. Munchen: J. F. Lehmann. 1914. pp. 528. Illus. M.5.

Technical Words and Phrases. An English-German and German-English Dictionary (2nd Edition). London: E. Marlborough and Co. 1913. pp. 237. Price 2s. 6d.

* See Review in this issue.

Die Wasserdrachen. J. Hofmann. Munchen: R. Oldenbourg. 1914. pp. 83. Figs. M.4.

Animal Flight: A Record of Observation. Dr. E. H. Hankin. London: Iliffe and Sons. 1913. pp. 405. Illus. 12s. 6d.

English Metric and Metric English Lengths. G. A. Rossetti. London: E. and F. N. Spon. 1913. pp. 80. 1s.

The Year Book of Wireless Telegraphy and Telephony. London: Marconi Press Agency. 1914. pp. 850. 2s. 6d.

PAMPHLETS.

Mitteilungen des Luftschiffbau Zeppelin Friedrichshafen. C. Dornier. 1912 and 1913. Reprints from *Zeitschrift fur Flugtechnik und Motorluftschiffahrt.*

Eddy Winds of Gibraltar. H. Harries. Reprint from *Royal Meteorological Society Journal*, January, 1914.

REVIEWS.

Flying: Some Practical Experiences. By Gustav Hamel and Chas. C. Turner. (London: Longmans, Green. 1914. Illus. pp. 341. 12s. 6d. net.)

We are happily past the time when anybody with sufficient self-assurance and a facile pen could get even reputable firms to publish their works (save the mark!) on aeronautics. Nowadays the public have no excuse for ignorance on this splendid subject—there are really good and informing books dealing with its every phase; but having, for my sins, read every aeronautical book that has appeared in Great Britain during the last seven years, I should certainly place "Flying," by Messrs. Hamel and Turner, at the head of any list drawn up for the guidance of the intelligent inquirer. The complaint is often heard that books on flight tell one everything except what one wants to know. This book has the supreme merit of silencing such a complaint. In fact it is so full of information one can find nowhere else that it is a pity the authors did not insist on an exhaustive index.

The contents are too varied for complete enumeration here, but mention may be made of the chapters on cross-country flying, choosing a machine, aerial touring, the cost of flying, aptitude for flying, learning to fly, hydro-aeroplaning, winds and eddies in the air, etc., etc.

There is also a remarkable chapter on the medical aspect of aviation, with a suggested scheme for the medical examination of pilots, by Dr. J. Elrick Adler. The book is fully illustrated with a splendid series of photographic reproductions.

THE AËRONAUTICAL JOURNAL for many years has had a reputation for impartial book reviews, a fact that was impressed upon me by the Editor when asking me for this notice. I am therefore obliged to conclude with a rebuke to the publishers for binding the book in such a weak and flimsy fashion that my copy, with the most reverent handling, came to pieces "in me 'and," as the housemaids say. Otherwise, I can find for "Flying" nothing but admiration and praise.

H.

Flugzeug Modelbau. By P. L. Bigenwald. (Berlin : Richard Carl Schmidt & Co. 1914. pp. 171. Illus.)

This work on Model Aeroplane Construction is clearly written and well illustrated. The author, or rather compiler, has spared no pains in getting together information from all quarters, and he has arranged his material in a readable form. The first three dozen pages having been devoted to a slight historical sketch of some of the forerunners, and to descriptions of one or two well-known types of present-day models—Farman, Olympia, Bleriot, etc.—the way is cleared for discussing in detail the methods followed and the materials used in model work.

Special attention is given to motors for models, and the sections relating to propellers are carefully handled. While "theory" is not overlooked throughout the main text, a special section at the end discusses "resistance," "centre of pressure," "centre of gravity," "stability," etc. Kites are given a chapter; gliders are treated to an appendix. The book is written in simple language and should become popular in model aeroplanist circles.

J. E. H.

Aéro-Manuel, 1914. By Ch. Faroux and G. Bonnet. (Paris : Dunod & Pinat. pp. 868. 12 francs.)

It is an imposing production. Perhaps nothing could better convey an idea of the magnificent development of the aviation industry, with its manifold ramifications. Moreover, contrary to the fashion in the majority of such year-books, it is really well produced, and its information on the whole reliable enough. A brief survey of the early history of aeronautics is followed by a detailed chronology of the events of recent years, extending to the end of June of last year. A number of tables, also arranged in chronological order, giving the principal cross-country flights and most notable airship and balloon ascents, with an exhaustive list of successive records and the results of the principal meetings complete this most useful section.

The next section is devoted to the military aeronautical establishment and organisation of the various Powers. Here, again, the arrangement is good and the information generally reliable.

There follows an excellent dictionary, a series of technical notes and tables, and finally an illustrated description of the various types of aeroplanes and motors arranged under the respective countries.

The subsequent section contains a very complete series of notes and tables dealing with the theory and practice of petrol engines.

The main part of the work is, however, contained in the latter half, which is simply invaluable. It consists, firstly, of a most complete world-wide directory of every name associated in whatsoever capacity with aerial navigation; next, of a classified list of constructors; and, finally, of a list of clubs, members, and pilots.

For the industry of the editors one can have nothing but praise. On the whole, this publication has no rival. There is certainly nothing like it in English—a defect which, let us hope, will soon be remedied.

Errors are bound to creep into a work of this nature, and some of these, revealed by a somewhat cursory examination, may well be enumerated, both for the benefit of unwary readers and with a view to their correction in future editions.

The chronology is good and accurate on the whole; but in some of their references to British events, the editors have gone astray. Thus, it is incorrect

XUM

that Sir George Cayley described a monoplane *with two propellers,* whose characteristic features are again found in Henson's later monoplane. The title of Coxwell's magazine should also be corrected to "The Balloon and Aeronautic Magazine." The solitary reference to Wenham is incorrect in every respect, while no mention is made of the foundation of the Aeronautical Society.

In the military section occur several serious mistakes, such as the statement that Captain Paine commands the Military Wing, that the "Mayfly" is under construction by Messrs. Vickers, and that the "Morning Post" airship and the "Clément-Bayard" may be regarded as training vessels.

Some really extraordinary mistakes have been made in the list of British aerodromes and airship stations. Thus we read with surprise that Brighton is a military centre, and possesses an iron airship shed 60 metres long by 20 metres in width, built in 1911! So, too, we are informed that at Wonnwood Scruls (sic) there are two iron airship sheds. This section requires drastic revision, as do the particulars given of British aeroplanes and engines.

J. H. L.

Taschenbuch der Luftflotten. By F. Rasch and W. Hormel. (Munich, 1914: J. F. Lehman. pp. 528. 5 Marks.)

Thoroughness and accuracy are well-known traits of the average German character, and these two qualities form conspicuous features of this really excellent handbook. The title, which may be translated as "Pocket-Book of the Air Fleets," precisely indicates both its nature and its scope. It opens with a detailed list of all the airships built throughout the world since 1910, and a record of their leading characteristics; this is followed by photographs, plans and silhouettes of the various types of these craft, and by a detailed list of airship sheds in the different countries. For the lucid arrangement of this section and for the exceedingly accurate information contained therein one can have nothing but praise. Careful examination has revealed no flaw and, more gratifying still, the particulars relating to England are correct—save only that the famous mythical airship shed at Brighton reappears—and the names accurately spelled.

There follows a similar exhaustive section devoted to aeroplanes and sea-planes, accompanied by excellent photographs and drawings, and though these latter are not always those of the latest machines, the date of their construction is always recorded with scrupulous honesty. Would that this practice were more widely followed!

The third section relates to aircraft motors, and lastly the authors give us in adequate detail a summary of the military and naval organisations as at present established in the various countries, and of the different aerial navigation acts and regulations. To complete the scheme, there are added a whole series of excellent tables of useful information.

In its own particular sphere this little handbook stands unrivalled. It possesses in full measure that characteristic which one demands before all others from a work of reference—reliability. The format, moreover, is handy; it really can be slipped into a pocket, for all its five hundred pages. This is its first year of issue; one may confidently predict for it a long life.

J. H. L.

CORRESPONDENCE.

The Editor, THE AËRONAUTICAL JOURNAL.

SIR,—In your review of "Aviation" you criticise my reference to the relative importance of the upper surface of a wing as "an unsupported statement." Pages 103 and 314 of "Aviation" may be mentioned as essentially relating to the subject and on the former I cite Eiffel and on the latter the Technical Report of the Advisory Committee as the sources of my information.

A. E. BERRIMAN.

[This is certainly so, but the statement quoted stands in a state of splendid isolation on p. 25. A footnote reference to pp. 103 and 314 would seem necessary.

I should like to add that I wrote "*bald* unsupported statements," but the printer made it appear as "bold," which is not quite the same thing.—THE REVIEWER.]